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XXXI. *Experimental Researches in Electricity.—Third Series.*

By Michael Faraday, *D.C.L. F.R.S. M.R.I. Fullerian Prof. Chem. Royal Institution, Corr. Mem. Royal Acad. of Sciences, Paris, Petersburgh, &c. &c.*[*]

§ 7. *Identity of Electricities derived from different Sources.*
§ 8. *Relation by Measure of common and Voltaic Electricity.*

§ 7. *Identity of Electricities derived from different Sources.*

265. THE progress of the electrical researches which I have had the honour to present to the Royal Society, brought me to a point at which it was essential for the further prosecution of my inquiries that no doubt should remain of the identity or distinction of electricities excited by different means. It is perfectly true that Cavendish[†], Wollaston[‡], Colladon[§] and others, have in turn removed some of the greatest objections to the acknowledgement of the identity of common, animal and voltaic electricity, and I believe that philosophers generally consider these electricities as really the same. But on the other hand it is also true, that the accuracy of Wollaston's experiments has been denied[||], and that one of them, which really is no proof of chemical decomposition by com-

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[*] From the Phil. Trans. for 1833, Part I. This paper was read before the Royal Society, Jan. 10 and 17, in the present year. Abstracts of the author's First and Second Series of Experimental Researches in Electricity, containing § 1 to § 6, will be found in Phil. Mag. and Annals, N.S. vol. xi. pp. 445, 447.

[†] Phil. Trans. 1776, p. 196.

[‡] Ibid. 1801, p. 434.

[§] *Annales de Chimie*, 1826, p. 62, &c.

[||] Phil. Trans. 1832, p. 282, note.

mon electricity (309. 327.), has been that selected by several experimenters as the test of chemical action (336. 346.). It is a fact, too, that many philosophers are still drawing distinctions between the electricities from different sources; or at least doubting whether their identity is proved. Sir Humphry Davy, for instance, in his paper on the Torpedo*, thought it probable that animal electricity would be found of a peculiar kind; and referring to that, in association with common electricity, voltaic electricity and magnetism, has said, "Distinctions might be established in pursuing the various modifications or properties of electricity in these different forms, &c." Indeed I need only refer to the last volume of the Philosophical Transactions to show that the question is by no means considered as settled†.

266. Notwithstanding, therefore, the general impression of the identity of electricities, it is evident that the proofs have not been sufficiently clear and distinct to obtain approbation from all those who were competent to consider the subject; and the question seemed to me very much in the condition of that which Sir H. Davy solved so beautifully,—namely,

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* Phil. Trans. 1829, p. 17. "Common electricity is excited upon non-conductors, and is readily carried off by conductors and imperfect conductors. Voltaic electricity is excited upon combinations of perfect and imperfect couductors, and is only transmitted by perfect conductors or imperfect conductors of the best kind. Magnetism, if it be a form of electricity, belongs only to perfect conductors; and, in its modifications, to a peculiar class of them[a]. Animal electricity resides only in the imperfect conductors forming the organs of living animals, &c."

† Phil. Trans. 1832, p. 259. Dr. Davy, in making experiments on the torpedo, obtains effects the same as those produced by common and voltaic electricity, and says that in its magnetic and chemical power it does not seem to be essentially peculiar,—p. 274; but he then says, p. 275, there are other points of difference; and after referring to them, adds, "How are these differences to be explained? Do they admit of explanation similar to that advanced by Mr. Cavendish in his theory of the torpedo; or may we suppose, according to the analogy of the solar ray, that the electrical power, whether excited by the common machine, or by the voltaic battery, or by the torpedo, is not a simple power, but a combination of powers, which may occur variously associated, and produce all the varieties of electricity with which we are acquainted?"

At p. 279 of the same volume of Transactions is Dr. Ritchie's paper, from which the following are extracts: "Common electricity is diffused over the surface of the metal;—voltaic electricity exists within the metal. Free electricity is conducted over the surface of the thinnest gold leaf as effectually as over a mass of metal having the same surface;—voltaic electricity requires thickness of metal for its conduction," p. 280: and again, "The supposed analogy between common and voltaic electricity, which was so eagerly traced after the invention of the pile, completely fails in this case, which was thought to afford the most striking resemblance," p. 291.

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[a] Dr. Ritchie has shown this is not the case, Phil. Trans. 1832, p. 294.

whether voltaic electricity in all cases merely eliminated, or did not in some actually produce, the acid and alkali found after its action upon water. The same necessity that urged him to decide the doubtful point, which interfered with the extension of his views, and destroyed the strictness of his reasoning, has obliged me to ascertain the identity or difference of common and voltaic electricity. I have satisfied myself that they are identical, and I hope the proofs I have to offer, and the results flowing from them, will be found worthy the attention of the Royal Society.

267. The various phænomena exhibited by electricity may, for the purposes of comparison, be arranged under two heads; namely, those connected with electricity of tension, and those belonging to electricity in motion. This distinction is taken at present not as philosophical, but merely as convenient. The effect of electricity of tension, at rest, is either attraction or repulsion at sensible distances. The effects of electrical currents may be considered as 1st, Evolution of heat; 2nd, Magnetism; 3rd, Chemical decomposition; 4th, Physiological phænomena; 5th, Spark. It will be my object to compare electricities from different sources, and especially common and voltaic electricities, by their power of producing these effects.

I. *Voltaic Electricity.*

268. *Tension.*—When a voltaic battery of 100 pairs of plates has its extremities examined by the ordinary electrometer, it is well known that they are found positive and negative, the gold leaves at the same extremity repelling each other, the gold leaves at different extremities attracting each other, even when half an inch or more of air intervenes.

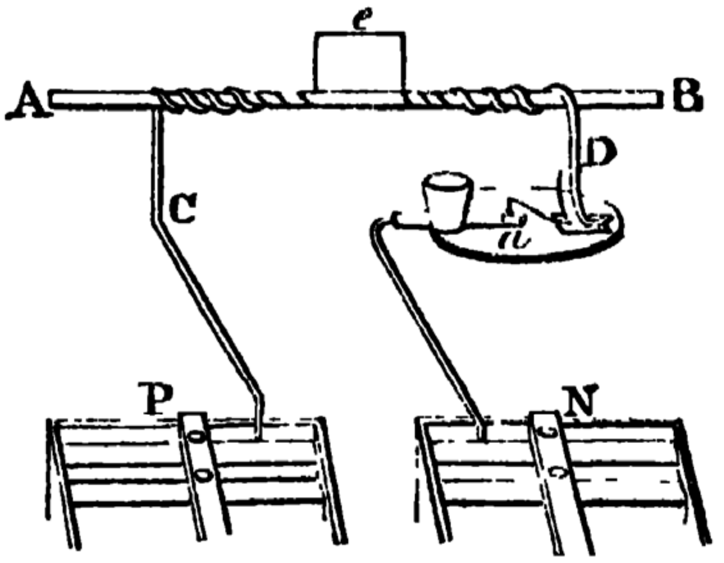
269. That ordinary electricity is discharged by points with facility through air; that it is readily transmitted through highly rarefied air; and also through heated air, as for instance a flame; is due to its high tension. I sought, therefore, for similar effects in the discharge of voltaic electricity, using as a test of the passage of the electricity either the galvanometer or chemical action produced by the arrangement hereafter to be described (312. 316.).

270. The voltaic battery I had at my disposal consisted of 140 pairs of plates four inches square, with double coppers. It was insulated throughout, and diverged a gold leaf electrometer about one third of an inch. On endeavouring to discharge this battery by delicate points very nicely arranged and approximated, either in the air or in an exhausted receiver, I could obtain no indications of a current, either by

magnetic or chemical action. In this, however, was found no point of discordance between voltaic and common electricity; for when a Leyden battery (291.) was charged so as to deflect the gold leaf electrometer equally, the points were found equally unable to discharge it with such effect as to produce either magnetic or chemical action. This was not because common electricity could not produce both these effects (307. 310.), but because when of such low intensity the quantity required to make the effects visible (being enormously great (371. 375.),) could not be transmitted in any reasonable time. In conjunction with the proofs of identity hereafter to be given, these effects of points also prove identity instead of difference between voltaic and common electricity.

271. As heated air discharges common electricity with far greater facility than points, I hoped that voltaic electricity might in this way also be discharged. An apparatus was therefore constructed (fig. 5.), in which AB is an insulated glass rod upon which two copper wires, C, D, are fixed firmly; to these copper wires are soldered two pieces of fine platina wire, the ends of which are brought very close to each other at *e*, but without touching; the copper wire C was connected with the positive pole of a voltaic battery, and the wire D with a decomposing apparatus (312. 316.), from which the communication was completed to the negative pole of the battery. In these experiments only two troughs, or twenty pairs of plates, were used.

Fig. 5.



272. Whilst in the state described, no decomposition took place at the point *a*, but when the side of a spirit-lamp flame was applied to the two platina extremities at *e*, so as to make them bright red-hot, decomposition occurred; iodine soon appeared at the point *a*, and the transference of electricity through the heated air was established. On raising the temperature of the points *e* by a blowpipe, the discharge was rendered still more free, and decomposition took place instantly. On removing the source of heat, the current immediately ceased. On putting the ends of the wires very close by the side of and parallel to each other, but not touching, the effects were perhaps more readily obtained than before. On using a larger voltaic battery (270.), they were also more freely obtained.

273. On removing the decomposing apparatus and inter-

posing a galvanometer instead, heating the points *e* as the needle would swing one way, and removing the heat during the time of its return (302.), feeble deflections were soon obtained: thus also proving the current through heated air; but the instrument used was not so sensible under the circumstances as chemical action.

274. These effects, not hitherto known or expected under this form, are only cases of the discharge which takes place through air between the charcoal terminations of the poles of a powerful battery, when they are gradually separated after contact. Here the passage is through heated air exactly as with common electricity, and Sir H. Davy has recorded that with the original battery of the Royal Institution this discharge passed through at least four inches of air*. In the exhausted receiver the electricity would *strike* through nearly half an inch of space, and the combined effects of rarefaction and heat was such upon the inclosed air as to enable it to conduct the electricity through a space of six or seven inches.

275. The instantaneous charge of a Leyden battery by the poles of a voltaic apparatus is another proof of the tension, and also the quantity, of electricity evolved by the latter. Sir H. Davy says†, "When the two conductors from the ends of the combination were connected with a Leyden battery, one with the internal, the other with the external coating, the battery instantly became charged, and on removing the wires and making the proper connexions, either a shock or a spark could be perceived: and the least possible time of contact was sufficient to renew the charge to its full intensity."

276. *In motion.* i. *Evolution of Heat.*—The evolution of heat in wires and fluids by the voltaic current is matter of general notoriety.

277. ii. *Magnetism.*—No fact is better known to philosophers than the power of the voltaic current to deflect the magnetic needle, and to make magnets according to *certain laws*; and no effect can be more distinctive of an electrical current.

278. iii. *Chemical decomposition.*—The chemical powers of the voltaic current, and their subjection to *certain laws*, are also perfectly well known.

279. iv. *Physiological Effects.*—The power of the voltaic current, when strong, to shock and convulse the whole animal system, and when weak to affect the tongue and the eyes, is very characteristic.

280. v. *Spark.*—The brilliant star of light produced by the

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* Elements of Chemical Philosophy, p. 153.

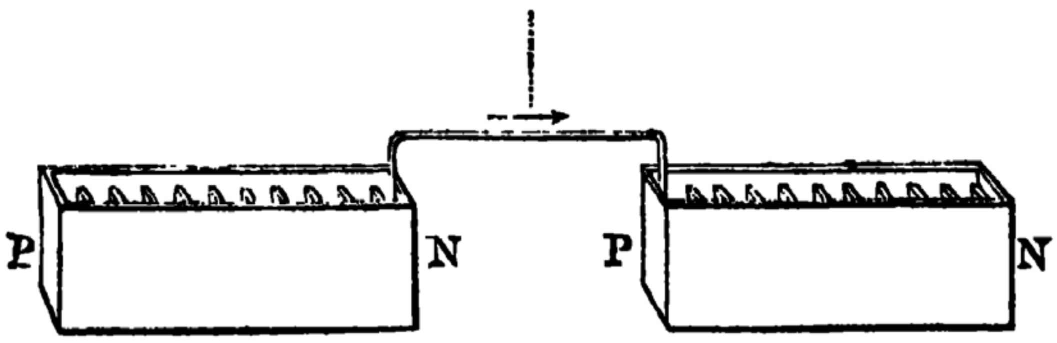
† Ibid. p. 154.

discharge of a voltaic battery is known to all as the most beautiful light that man can produce by art.

281. That these effects may be almost infinitely varied, some being exalted whilst others are diminished, is universally acknowledged; and yet without any doubt of the identity of character of the voltaic currents thus made to differ in their effect. The beautiful explication of these variations afforded by Cavendish's theory of quantity and intensity requires no support at present, as it is not understood to be doubted.

282. In consequence of the comparisons that will hereafter arise between wires carrying voltaic and ordinary electricities, and also because of certain views of the condition of a wire or any other conducting substance connecting the poles of a voltaic apparatus, it will be necessary to give some definite view of what is called the voltaic current, in contradistinction to any supposed peculiar state of arrangement, not progressive, which the wire or the electricity within it may be supposed to assume. If two voltaic troughs PN, P′N′, fig. 1. be symmetrically arranged and insulated, and the ends NP′ connected by a wire, over which a magnetic needle is suspended, the wire will exert no effect over the needle; but immediately that the ends PN′ are connected by another wire, the needle will be deflected, and will remain so as long as the circuit is complete. Now if the troughs merely act by causing a peculiar arrangement in the wire either of its particles or its electricity, that arrangement[*] constituting its electrical and magnetic state, then the wire NP′ should be in a similar state of arrangement *before* P and N′ were connected, to what it is afterwards, and should have deflected the needle, although less powerfully, perhaps to one half the extent which would result when the communication is complete throughout. But if the magnetic effects depend upon a current, then it is evident why they could not be produced in *any* degree before the circuit was complete; because prior to that no current could exist.

Fig. 1.



283. By *current,* I mean anything progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations, or, speaking still more ge-

nerally, progressive forces. By *arrangement*, I understand a local adjustment of particles, or fluids, or forces, not progressive. Many other reasons might be urged in support of the view of a *current* rather than an *arrangement*, but I am anxious to avoid dilating unnecessarily upon what can be supplied by others at the moment.

II. *Ordinary Electricity.*

284. By ordinary electricity I understand that which can be obtained from the common machine, or from the atmosphere, or by pressure, or cleavage of crystals, or by a multitude of other operations; its distinctive character being that of great intensity, and the exertion of attractive and repulsive powers, not merely at sensible but at considerable distances.

285. *Tension.* The attractions and repulsions at sensible distances, caused by ordinary electricity, are well known to be so powerful in certain cases, as to surpass, almost infinitely, the similar phænomena produced by electricity, considered as of other kinds. But still those attractions and repulsions are exactly of the same nature as those already referred to under the head *Tension, Voltaic electricity* (268.); and the difference in degree between them is not greater than often occurs between cases of ordinary electricity only. I think it will be unnecessary to enter minutely into the proofs of the identity of this character in the two instances. They are abundant; are generally admitted as good; and lie upon the surface of the subject: and whenever in other parts of the comparison I am about to draw, a similar case occurs, I shall content myself with a mere announcement of the similarity, expanding only upon those parts where the great question of distinction or identity is still controverted.

286. The discharge of common electricity through heated air is a well-known fact. The parallel case of voltaic electricity has already been described (272, &c.).

287. *In motion.* i. *Evolution of Heat.* The heating power of common electricity when passed through wires or other substances, is perfectly well known. The accordance between it and voltaic electricity is in this respect complete. Mr. Harris has constructed and described* a very beautiful and sensible instrument on this principle, in which the heat produced in a wire by the discharge of a mere spark of common electricity is readily shown, and to which I shall have occa-

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* Philosophical Transactions, 1827, p. 18. Edinburgh Transactions,1831. Harris on a New Electrometer, &c. &c.

sion to refer for experimental proof in a future part of this paper (344.).

288. ii. *Magnetism.* Voltaic electricity has most extraordinary and exalted magnetic powers. If common electricity be identical with it, it ought to have the same powers. In rendering needles or bars magnetic, it is found to agree with voltaic electricity, and the *direction* of the magnetism, in both cases, is the same; but in deflecting the magnetic needle, it has been found deficient, so that sometimes the power has been denied altogether, and at other times distinctions have been hypothetically assumed for the purpose of avoiding the difficulty*.

289. M. Colladon, of Geneva, considered that the difference might be due to the use of insufficient quantities of common electricity in all the experiments before made upon this point; and in a memoir read to the *Académie des Sciences* in 1826†, describes experiments, in which, by the use of a battery, points, and a delicate galvanometer, he succeeded in obtaining deflections, and thus establishing identity in that respect. MM. Arago, Ampere, and Savary are mentioned in the paper as having witnessed a successful repetition of the experiments. But as no second witness of these effects has come forward, MM. Arago, Ampere, and Savary, not having themselves published (that I am aware of,) their acceptance of the results, and as others have not been able to obtain the effects, M. Colladon's conclusions have been by some doubted or denied; and an important point with me was to establish their accuracy, or remove them entirely from the received body of experimental evidence. I am happy to say that my results fully confirm those by M. Colladon, and I should have had no occasion to describe them, but that they are essential as proofs of the accuracy of the final and general conclusions I am enabled to draw respecting the magnetic and chemical action of electricity, (360. 366. 367. 377. &c.).

290. The plate electrical machine I have used is fifty inches in diameter; it has two sets of rubbers; its prime conductor consists of two brass cylinders connected by a third, the whole length being twelve feet, and the surface in contact with air about 1422 square inches. When in good excitation, one revolution of the plate will give ten or twelve sparks from the conductors, each an inch in length. Sparks or flashes from ten to fourteen inches in length may easily be drawn from the

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* Demonferrand's *Manuel d'Electricité dynamique*, p. 121.

† *Annales de Chimie*, xxxiii. p. 62.

conductors. Each turn of the machine, when worked moderately, occupies about $\frac{4}{5}$ths of a second.

291. The electric battery consisted of fifteen equal jars. They are coated eight inches upwards from the bottom, and are twenty-three inches in circumference, so that each contains one hundred and eighty-four square inches of glass, coated on both sides, independent of the bottoms, which are thicker glass, and contain each about fifty square inches.

292. A good discharging train was arranged by connecting metallically a sufficiently thick wire with, first, the metallic gas pipes of the house, then with the metallic gas pipes belonging to the public gas works of London; and finally, with the metallic water pipes of London. It was so effectual as to carry off instantaneously electricity of the feeblest tension, even that of a single voltaic trough, and was essential to many of the experiments.

293. The galvanometer was one or the other of those formerly described (87. 205.), but the glass jar covering it and supporting the needle was coated inside and outside with tinfoil, and the upper part (left uncoated, that the motions of the needle might be examined,) was covered with a frame of wire-work, having numerous sharp points projecting from it. When this frame and the two coatings were connected with the discharging train (292.), an insulated point or ball, connected with the machine when most active, might be brought within an inch of any part of the galvanometer, yet without affecting the needle within by any ordinary electrical attraction or repulsion.

294. In connexion with these precautions, it may be necessary to state that the needle of the galvanometer is very liable to have its magnetic power deranged, diminished, or even inverted by the passage of a shock through the instrument. If the needle be at all oblique in the wrong direction to the coils of the galvanometer, when the shock passes, effects of this kind are sure to happen.

295. It was to the retarding power of bad conductors, that I first looked with the hope of being able to make common electricity assume more of the characters and power of voltaic electricity, than it is usually supposed to have.

296. The coating and armour of the galvanometer were first connected with the discharging train (292.); the end B (87.) of the galvanometer wire was connected with the outside coating of the battery, and then both these with the discharging train; the end A of the galvanometer wire was connected with a discharging rod by a wet thread four feet long;

and finally, when the battery had been positively charged by about forty turns of the machine, it was discharged by the rod and the thread through the galvanometer. The needle immediately moved.

297. During the time that the needle completed its vibration in the first direction and returned, the machine was worked, and the battery charged; and when the needle in vibrating resumed its first direction, the discharge was again made through the galvanometer. By repeating this action a few times, the vibrations soon extended to above 40° on each side of the line of quiescence.

298. This effect could be obtained at pleasure. Nor was it varied, apparently, either in direction or degree, by using a short thick string, or even four short thick strings in place of the long fine thread. With a more delicate galvanometer, an excellent swing of the needle could be obtained by one discharge of the battery.

299. On reversing the galvanometer communications so as to pass the discharge through from B to A, the needle was equally well deflected, but in the opposite direction.

300. The deflections were in the same direction as if a voltaic current had been passed through the galvanometer, i. e. the positively charged surface of the electric battery coincided with the positive end of the voltaic apparatus (268.), and the negative surface of the first with the negative end of the latter.

301. The battery was then thrown out of use, and the communications so arranged that the current could be passed from the prime conductor, by the discharging rod held against it, through the wet string, through the galvanometer coil, and into the discharging train (292.), by which it was finally dispersed. This current could be stopped at any moment, by removing the discharging rod, and either stopping the machine or connecting the prime conductor by another rod with the discharging train; and could be as instantly renewed. The needle was so adjusted, that whilst vibrating in moderate and small arcs, it required time equal to twenty-five beats of a watch to pass in one direction through the arc, and of course an equal time to pass in the other direction.

302. Thus arranged, and the needle being stationary, the current, direct from the machine, was sent through the galvanometer for twenty-five beats, then interrupted for other twenty-five beats, renewed for twenty-five beats more, again interrupted for an equal time, and so on continually. The needle soon began to vibrate visibly, and after several

alternations of this kind, the vibration increased to 40° or more.

303. On changing the direction of the current through the galvanometer, the direction of the deflection of the needle was also changed. In all cases the motion of the needle was the same in direction as that caused by the use of an electric battery or a voltaic trough (300.).

304. I now rejected the wet string, and substituted a copper wire, so that the electricity of the machine passed at once into wires communicating directly with the discharging train, the galvanometer coil being one of the wires used for the discharge. The effects were exactly those obtained above (302.).

305. Instead of passing the electricity through the system, by bringing the discharging rod at the end of it into contact with the conductor, four points were fixed on to the rod; when the current was to pass, they were held about twelve inches from the conductor, and when it was not to pass, they were turned away. Then, except with this variation operating as before (302.), the needle was soon powerfully deflected, and in perfect consistency with the former results. Points afforded the means by which Colladon, in all cases, made his discharges.

306. Finally, I passed the electricity first through an exhausted receiver, so as to make it there resemble the aurora borealis, and then through the galvanometer to the earth; and it was found still effective in deflecting the needle, and apparently with the same force as before.

307. From all these experiments, it appears that a current of common electricity, whether transmitted through water or wire, or rarefied air, or by means of points in common air, is still able to deflect the needle; the only requisite being, apparently, to allow time for its action: that it is, in fact, just as magnetic in every respect as a voltaic current, and that in this character therefore no distinction exists.

308. Imperfect conductors, as water, brine, acids, &c. &c. will be found far more convenient for exhibiting these effects than other modes of discharge, as by points or balls; for the former convert at once the charge of a powerful battery into a feeble spark, or rather continuous current, and involve little or no risk of deranging the magnetism of the needles (294.).

[To be continued.]

XXXII. *On the Inflection of Light, in Reply to* Professor Powell. *By* JOHN BARTON, *Esq.**

I AM much indebted to Professor Powell for the candid and friendly manner in which he has commented on the objections to the undulatory theory of light, which I adduced in my former communication. On the present occasion I propose, first, to make a few remarks in explanation or justification of what I before advanced; and then to suggest another hypothesis, which I persuade myself is capable of explaining the phænomena of inflection more completely and satisfactorily than that of Fresnel.

The first question at issue between the undulationists and myself relates to a matter of fact. Newton found that when a beam of light is suffered to fall on a very narrow slit, the beam parts in the middle, leaving a dark space between the two portions into which it is divided. This result agrees with my own observation. Professor Airy, on the contrary, finds the centre of the spectrum always relatively bright, however narrow the slit through which the light passes. I have no wish to disparage Professor Airy's authority; but I must be permitted to prefer the testimony of my own senses, supported as it is by the observations of Newton, and even, I think I may add, by the observations of Professor Powell. For although this gentleman has not succeeded in reproducing the phenomena under the exact form described by me, and delineated in my last communication, he has said enough to convince every one who reflects on the subject, of the possibility of so succeeding when the experiment is repeated with due precaution. "When," says he, "the edges approached very nearly, I observed something like the appearance described by Mr. Barton, but with this difference, that the boundaries of the dark space, instead of being continuous, diverged either way into the shadow in hyperbolic lines." Now, I am sure that if Professor Powell will reconsider the question, he will see that, from the nature of the hyperbola, there must, on his own showing, be a certain width of interval between the two knife-blades, giving a result such as I have described. In fact, it is easy, from the hyperbolic form of the lines, combined with Newton's observation that the dark space begins to appear when the interval between the two blades is less than the four-hundredth part of an inch, to assign the precise distance at which the blades must be placed, and the degree of curvature they must possess, in order to give a dark space of any required figure and dimensions. I need not dwell on this point, since the details of the computation will be readily supplied by any one versed in mathematics. But I may add, that if Professor Powell will employ blades of less curvature, he will find the experiment succeed more readily. Those employed by him in repeating my experiment had, if I do not mistake, so inconsiderable a radius of curvature as to reduce the dimensions of the dark space within a limit too small to be conveniently observed.

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* Communicated by the Author: see our preceding volume, p. 424.

It would seem, indeed, from his subsequent observations, that Professor Powell is not quite sure whether to admit or not the correctness of Newton's observation; for after saying that he cannot succeed in obtaining the results described by me, he goes on to assign certain considerations which may serve, he thinks, to reconcile the phænomena with the undulatory hypothesis, *even supposing* the facts to be such as I have asserted. The formulæ of Fresnel, he says, always suppose the opening through which the light enters the darkened chamber to be a mathematical point: whereas Newton employed for this purpose a hole a quarter of an inch in breadth. But the experiment succeeds with me equally well, whether the opening in question be large or small, while it fails equally in either case with Professor Airy; this cannot, therefore, be the source of the different results obtained by us. Indeed it appears quite incredible that the enlargement or contraction of the aperture by which the light enters the chamber can produce a difference *of such a kind* as supposed by Professor Powell. That a change in the dimensions of the aperture should affect the breadth or intensity of the dark space may be readily imagined; but that such a change should convert light into darkness appears inconceivable.

The second observation of Professor Powell under this head I am apprehensive I do not fully comprehend. He says, "It results from the well-known fact of the enlargement of the shadows of the two edges beyond their geometrical boundaries, that these shadows will coalesce before the edges meet;......and this seems very likely to have been the real result observed by Newton." Surely the formulæ of Fresnel are intended to comprise the whole of the phenomena, including among the rest that enlargement of the shadow to which Professor Powell alludes. If so, it cannot be allowable to employ this enlargement as an after correction to the results of calculations deduced from those formulæ.

With regard to another objection advanced by me against the theory of Fresnel,—its inconsistency with the observations of Newton and Biot on the distance at which the first of the dark bands cross one another in the centre of the spectrum,—Professor Powell thinks these observations not to be relied on, as being of earlier date, and performed with apparatus less susceptible of accuracy than that employed by Fresnel. I must observe, however, that the question at issue does not rest on the comparative accuracy of the methods of observation employed. Fresnel has not recorded any experiments made under circumstances similar to those which gave to Newton and Biot results so much at variance with his theory. He examined in two cases only the distance at which the first dark bands are found to intersect. In these two cases the width of the slit was, respectively, two millimetres, and one millimetre and a half*; whereas the discordant results given by the observations of Newton and Biot relate to widths below one millimetre, or the twenty-fifth part of an inch. Even, therefore, if the observations of Fresnel were as far superior in accuracy to those of his predecessors as Professor Powell assumes, it would not be decisive in favour of his theory. But I confess I cannot see any sufficient ground for attributing to the observations of Fresnel such unmeasured superiority as has been supposed. I cannot think that if Fresnel deserves all confidence to the hundredth part of a millimetre, Newton and Biot are unworthy of being trusted to the tenth part of a millimetre, as Professor Powell's reasoning implies. I cannot think that the accuracy of different series of observations is to be estimated solely by their relative antiquity. Fresnel certainly introduced one important improvement into the method of observation;—he received the rays on a lens, instead of receiving them on a sheet of paper, like Newton, or on a piece of roughened glass, like Biot: he was thus enabled to measure the intervals between the fringes by a micrometer,—a method which admits of greater accuracy than the earlier method of taking off the distance by a pair of compasses. But let it be observed that in the particular class of phænomena at present under consideration, the advantage gained by this improvement in the method of observation ceases altogether; since the question relates, not to the width of the interval between two fringes, but to the place of their intersection. With regard to another important point, the measurement of the width of the slit, there is some reason to think Newton's method of observation preferable to Fresnel's. I do not, indeed, find in Fresnel's memoir any account of the manner in which this was accomplished in his experiments; but Newton has described the method employed by himself. Having placed his knife-blades with their edges (which were ground "truly straight,") facing each other, and their points meeting at a given small angle, he computed the interval between them at any distance from that angle, by the problem of similar triangles. This method appears susceptible of greater accuracy than any process of direct measurement. If, therefore, Fresnel employed some such direct means, I should be disposed to place greater confidence in the observations of Newton, who was no less remarkable for accuracy as an experimentalist than for genius as a mathematician. Professor Airy, indeed, finds Newton's measurements bad, because, "according to theory, when the breadth of the aperture is very small, the distance should vary as the square of the breadth," which is not found to be the case in Newton's observations. But I would ask, Is not this taking for granted the very thing to be proved? When we are seeking to bring to test the soundness of a theory by comparing it with the results of observation, can it be allowable to measure the accuracy of those very observations by their accordance or discordance with the theory in question?

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* *Mémoires de l'Institut,* tome v. pp. 398 and 437.

The results obtained by Newton and Biot display, I think, strong internal evidence that their discrepancy with the theory of Fresnel is not attributable solely to errors of observation. It will be observed, on examining those results, that when the width of the slit exceeds the thirtieth part of an inch, or thereabouts, the observations are not greatly at variance with the theory; but in proportion as the width is reduced below this limit, the discordance between theory and observation increases; and at length, when the width is less than the four-hundredth part of an inch, it becomes a difference, not only in degree, but in kind; the theory indicating that the spectrum should be brightest at the centre, while observation shows that this centre is quite dark. Looking at this progressively increasing discrepancy between the conclusions of theory and the observed phænomena, it is difficult to avoid suspecting that it depends on some defect in the theory itself.

In addition to these objections to the theory of Fresnel, founded on its want of agreement with observed phænomena, I suggested, in my former communication*, an objection of another kind. I endeavoured to show that the origin of the undulations supposed to constitute light is placed in this theory at a wrong point. Fresnel and his followers placed this origin at the point of intersection—whether a pin-hole through which the rays enter the darkened chamber, or the focus of a lens occupying the same position. To me it appears that the true origin of the undulations should be placed at the sun's surface. I am sure, from the language of Professor Powell's reply, that I did not explain myself on this head so fully and explicitly as I ought. He says, "If the aperture be very small, it is shown by theory that the new wave diverging from it, which is produced by the sum of all the small waves belonging to the original waves from RR′, &c., will be equally strong in all directions; and A in this case is the real centre and origin from which the new undulations commence."

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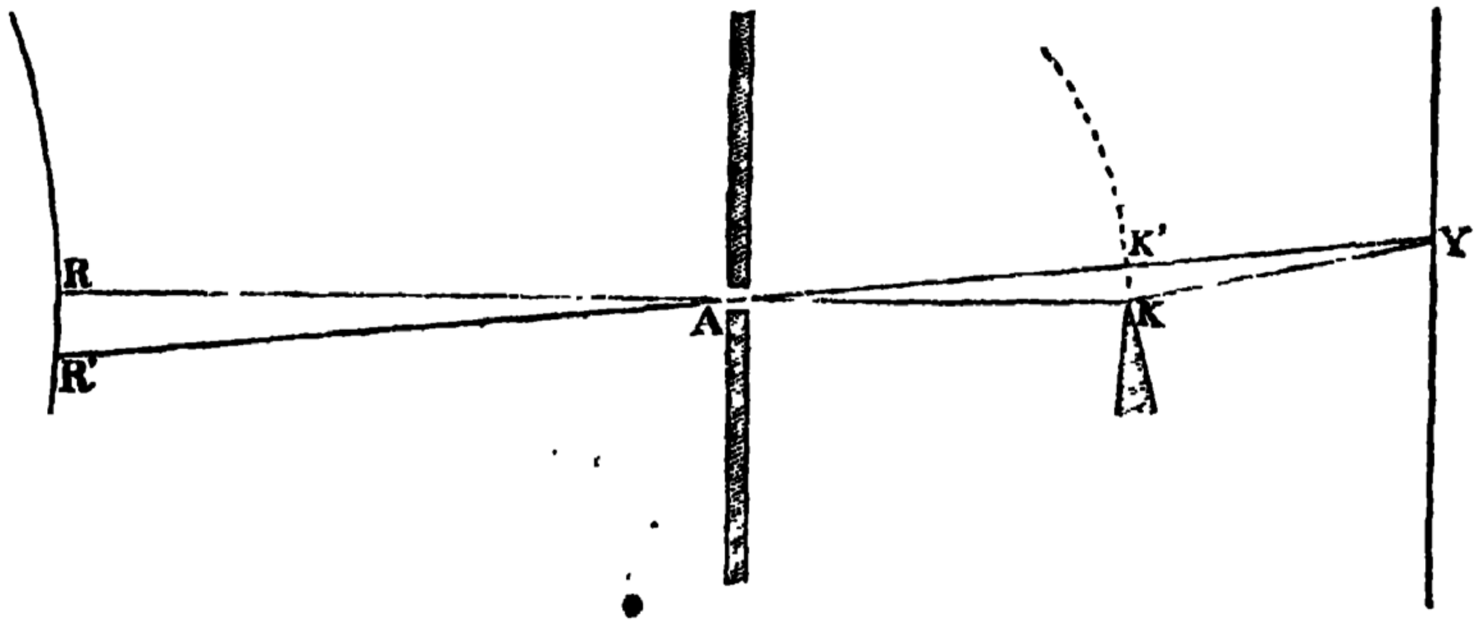
* See Lond. and Edinb. Phil. Mag. vol. ii. p. 263.—Edit.

In our sun, it is no doubt true, as observed by Professor Powell, that the wave diverging from the point of intersection "is produced by the sum of all the small waves belonging to the original waves from RR′ &c." It is no doubt true, on the principles of the undulatory theory, that the particles of æther composing the wave R′A, on arriving at the point A are agitated by a multitude of impulses derived from other points of the sun's surface; the succeeding portion AY may therefore be considered as the joint result of all these impulses. But inasmuch as the disturbing forces in question are infinite in number, and infinitely various in force and direction, they completely counteract each other's operation; and the practical result is, that the wave emanating from the point R′ goes forward to its ultimate destination at Y, precisely as if it had not been crossed in its path by any other rays whatever. Such at least was the doctrine of Huygens, the great author of the undulatory theory. If the undulationists of the present day have seen it necessary to abandon or modify the principles of their master, it appears incumbent on them to assign some reason for the change, and to show how it may be reconciled with the laws which are admitted to regulate the undulations of elastic media.



According to Young, the point Y will always be of double brightness if the paths of the two rays AY, AKY differ in length by an integral number of undulations. But it is evident that the chances are infinite against the two rays RA, R′A, arriving at the point A in the same state: their conditions must be as diversified as the lengths of the paths which they have severally traversed in arriving from the sun. What then is the nature of the force which operates upon them at A, the point of intersection, to bring them, or one of them, into a new condition? Suppose, for instance, that the wave forming the ray RA, on arriving at the point A, happens to be at its maximum of advance, while the wave forming the ray R′A, on arriving at the same point, happens to be at its maximum of retrogression. By the theory of Huygens, each will go forward in its course quite independently, as if the other did not exist. How happens it, then, that on arriving at K and K′ they are found in one and the same state? One or both of them must have experienced an acceleration, or a retardation, such as to make up together the length of half an undulation. How has this been accomplished?

In the theory of Young, respect is had only to the two rays RA, R′A, on whose interference the colour of the point Y is supposed to depend. In the theory of Fresnel, the new wave diverging from A is supposed, as Professor Powell truly says, to be produced by the sum of all the small waves belonging to the original waves from R R′, &c. But this, though a more accurate way of stating the question than Young's, gives in practice very nearly the same results; inasmuch as those parts only of the wave entering the chamber which follow the direction of the lines AY, AKY, do in fact constitute *effectual rays* ("*rayons efficaces*"), as Fresnel has himself observed. It is essential to his theory that every part of the hemispherical wave K′K should be found in one and the same condition, as it is to the theory of Young that the two individual rays should be found in like condition at K′ and K. The simple question which I would ask of the undulationists is, how has this equality of condition been brought about? I do not see how it is possible, on their theory, to give an answer to this question.

If, however, we suppose light to consist of material particles, endued with a mutually repulsive force, and so constituting an elastic fluid of great tenuity, the question just proposed may be solved without difficulty: for that such a fluid in passing through a narrow aperture, when pressed against an opposing surface, will be thrown into a series of undulatory movements, may be shown, I think, either by reference to the known laws of the motion of fluids, or by direct experiment. Take a tube, an inch or more in diameter, closed at one end, excepting a small orifice or slit, and blow strongly through it against the flame of a candle. The flickering motion of the flame will show that the air does not issue from the orifice in an uniform stream, but in a succession of rapidly alternating movements. This fact was observed by Dr. Young, and described by him in a paper published in the Philosophical Transactions for 1800. The mode of observation that he employed was, indeed, somewhat different from that here described: he contrived to render the issuing current of air visible by combining it with smoke. If the pressure is increased, a whistling sound is produced; affording another kind of proof that the air in passing through the orifice is thrown into a state of undulation.

Now, if the particles of light be supposed to constitute an elastic fluid, it is evident that a phænomenon of the same kind must take place whenever a stream of this fluid impinges upon a narrow aperture; and in this case the formulæ of Fresnel will be applicable, with some modification, to the hypothesis here proposed, as well as to the hypothesis which supposes light to consist in the vibrations of an imaginary æther; and with this advantage, that an obvious reason is assigned why the undulations are found to have their origin at the aperture by which the light enters: for which no reason, as it appears to me, can be given on the opposite theory.

By the help of the same principle it might also be explained, I think, why a beam of light passing through a very narrow slit, is separated into two portions, leaving a dark space between them; and why the formulæ of Fresnel are found to fail in approaching this limit. But it would, perhaps, be out of place at present to enlarge on these questions, which are not likely to excite much interest while the theory on which they rest is not yet recognised or established. I could bring forward other considerations in abundance which appear to me decisive against the undulatory hypothesis; but it seems preferable thoroughly to investigate and discuss, in the first instance, the circumstances of one or two classes of phænomena, rather than to lose ourselves in a wider circle of reasoning.

I cannot conclude without again expressing my sense of the friendly spirit in which Professor Powell has criticized my former observations. I should be fastidious indeed, if I did not perceive and acknowledge that his remarks deserve to be considered in no other light than as intended, and as calculated, "to promote the common cause of scientific truth."

XXXIII. *On* Bernoulli's *Solution of the Problem of Shortest Twilight.* *By* T. S. DAVIES, *Esq. F.R.SS. L. & E., F.R.A.S., &c.*[*]

THE problem of the shortest twilight was first solved, and very elegantly too, by Nunez, in a little quarto tract of 142 pages, printed at Lisbon in 1542; and it has engaged the attention of many distinguished authors since that time. John and James Bernoulli as well as D'Alembert and L'Hôpital discussed the problem: but the researches of the two illustrious brothers are nowhere, that I know of, to be found in print. The result is, indeed, stated in one of John Bernoulli's letters to the *Journ. des Savans*: but he says nothing that can lead us to discover the particular details of his solution. He however certainly bestowed a good deal of trouble upon it; as he says, "*J'ai résolu le problême de trouver géométriquement le jour du plus petit crépuscule, ce qui a occupé mon frère, Professeur de Mathématique à Bâle, et moi, depuis plus de cinq ans, sans en pouvoir venir à bout.*" He tells us, too, that he effected his solution by means of the differential calculus. D'Alembert, in the *Encyclopédie Méthodique* (Art. CRÉPUSCULE), undertook to give a complete solution on the supposed plan of Bernoulli: but he obtained also an equation of the fourth degree, the roots of which embarrassed him considerably. Two of the roots, it is true, he dismissed from his formula pretty readily; but not, as it appears to me, upon grounds altogether satisfactory. He does not attempt to show either how they came there or what was their actual signification; nor does he even attempt to do so with the remaining root of the remaining pair, though he employs more than three folio columns in showing (by special instead of general reasonings) that it does not belong to the minimum, and he even says, and attempts to prove, that it does not refer to the maximum twilight problem.

The intricacy of D'Alembert's solution seems to have deterred successive geometers from attempting to develop in a more advantageous form the probable method of Bernoulli, these being the only solutions (L'Hôpital's perhaps excepted, of which I cannot speak, not having his book within reach,) which I have found, in which the modern method of maxima and minima is employed. Every other solution with which I am acquainted proceeds upon certain geometrical considerations derived from the nature of the figure itself rather than from the equations of the problem: and it must be admitted that in point of facility these have very considerably the advantage over the other method. Had, therefore, the mere solution of the problem been the object to be principally sought for, there can be no doubt that it is already rendered as simple as it ever can be; and a very ample list of such solutions may be consulted in Professor Leybourn's edition of that valuable but unpretending little periodical, the Ladies' Diary, vol. iv. p. 314. We find there the solutions of Messrs. Ivory, Wallace, and Skene, as well as the very amplified discussions of Delambre, as given both in his *Astronomie*, and in his *Histoire de l'Astronomie Ancienne.* Two other solutions by Hachette, printed in the Correspondence of the Polytechnic School, vol. ii.; and others still, by Keill, Lemonnier, Maupertuis, Emerson, Mauduit, Cagnoli, &c., may be found in the respective writings of those authors. All these, however, are of the kind which may be called geometrical, in contradistinction to the method of Bernoulli and D'Alembert.

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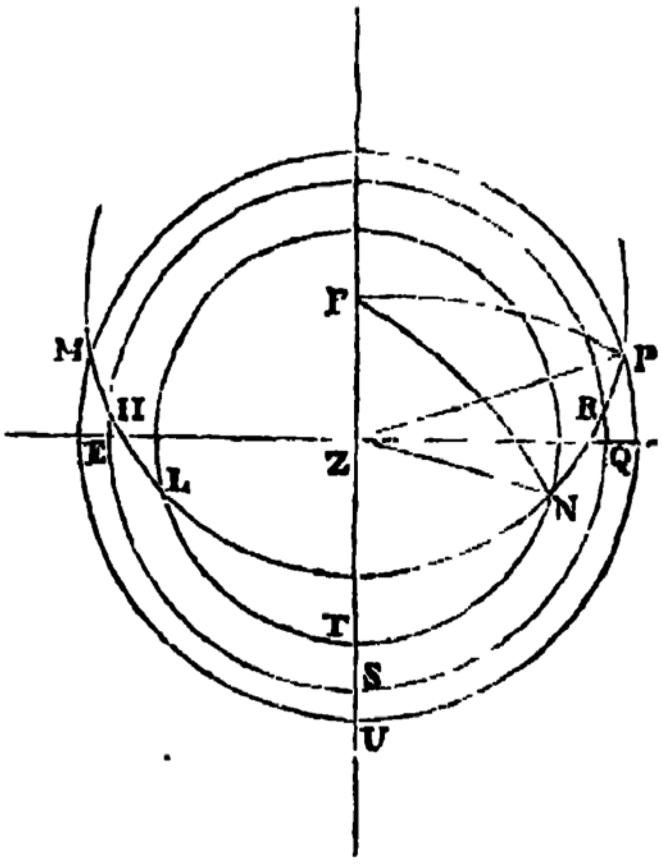
[*] Communicated by the Author.

Of these, Mr. Skene's is the only one which takes the problem in the more general form, in which another almacantar is substituted for the horizon of the common problem; and his solution is truly elegant as a specimen of the method of research which he adopted. Unfortunately it confined him to the simple question of the minimum, and he also was thereby led to give an incomplete solution, compared with that furnished by the calculus. The two cases, the maximum and the minimum, are so intimately connected that they present themselves naturally as the extreme cases of the question, *When the duration of twilight is given, to find the declination*; and they appear together in the same final equation interwoven in a single formula, and demanding in all respects equal attention. On this account, elegant as the solution of Mr. Skene is, it does not remove the necessity for reconsidering the problem under another aspect.

Some time ago, when composing a paper (since published in vol. xii. of the Edinburgh Transactions,) on the application of great-circle co-ordinates to the investigation of spherical loci, I was led to examine a considerable number of those problems which had been subjects of discussion, by the older methods of treating spherical curves—and the problem of shortest twilight amongst the rest. As, however, that problem did not properly appertain to my plan, I relinquished the inquiry for others more interesting to me at that moment, and laid aside the few memoranda which I had made concerning it. A day or two ago my attention was accidentally recalled to the subject; and as in those memoranda the problem had been viewed under its most general aspect, and the true character of all the roots of the equation assigned,—as, moreover, the problem is one of some historical celebrity, and its solution calculated to enforce an important truth,—on all these accounts its occupying a place in the Philosophical Magazine may perhaps be justified.

PROBLEM.—*Let* EHRQ *be the horizon of the place;* Z *the zenith;* P *the pole;* ESQ *the equator;* MUP′, LNT *any two almacantars;* LNPM *a circle of declination; then it is required to find the position or polar distance of* LNPM, *so that the arc of it intercepted between the given almacantars may be a maximum or a minimum.*



Such is the more general geometrical statement of the problem in the aspect under which the Bernoullis viewed it: the angle NPP′ is the measure of the time elapsed between the sun's passing the two almacantars. In the common problem for one of these almacantars is substituted the horizon itself.

Let NP′ be the intercepted portion of the circle of declination. Denote the colatitude, or PZ, by λ: put $\varrho, \varrho_{\prime}, \varrho_{\prime\prime}$ the spherical radii of the circles LRN, LTN, and MUP′ respectively: and denote the angles ZPP′ and ZPN by $\theta_{\prime\prime}$ and $\theta_{\prime}$. Then,

$$\theta_{\prime} = \cos^{-1} \frac{\cos \varrho_{\prime} - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda}$$

$$\theta_{\prime\prime} = \cos^{-1} \frac{\cos \varrho_{\prime\prime} - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda}$$

From which we have the polar angle or measure of the time,

$$\theta_{\prime\prime} - \theta_{\prime} = \cos^{1-} \frac{\cos \varrho_{\prime\prime} - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda}$$

$$- \cos^{1-} \frac{\cos \varrho_{\prime} - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda} \quad \ldots\ldots\ldots\ldots\ldots \quad (1)$$

But that the time may be a maximum or a minimum, we must have

$$d \left\{ \cos^{1-} \frac{\cos \varrho_{\prime\prime} - \cos \varrho \cos \lambda.}{\sin \varrho \sin \lambda} \right.$$

$$\left. \cos^{1-} \frac{\cos \varrho_{\prime} - \cos \varrho \cos \lambda}{\sin \varrho \sin \lambda} \right\} = 0 \ldots\ldots (2)$$

In this expression ϱ is the variable, and the differentiation being performed with respect to it, we have, after transposition,

$$\frac{(\cos\lambda - \cos\varrho_{,}\cos\varrho)\operatorname{cosec}\varrho}{\sqrt{(\sin^2\lambda - \cos^2\varrho_{,}) + 2\cos\varrho_{,}\cos\lambda\cos\varrho - \cos^2\varrho}}$$

$$= \frac{(\cos\lambda - \cos\varrho_{,,}\cos\varrho)\operatorname{cosec}\varrho}{\sqrt{\sin^2\lambda - \cos^2\varrho_{,,} + 2\cos\varrho_{,}\cos\lambda\cos\varrho - \cos^2\varrho}} \ldots (3)$$

Squaring these to take the radical from the denominator, and transposing all to one side, it takes the form

$$0 = \operatorname{cosec}^2\varrho\left\{\frac{(\cos\lambda - \cos\varrho_{,}\cos\varrho)^2}{\sin^2\lambda - \cos^2\varrho + 2\cos\lambda\cos\varrho_{,}\cos\varrho - \cos^2\varrho}\right.$$

$$\left. - \frac{(\cos\lambda - \cos\varrho_{,,}\cos\varrho)^2}{\sin^2\lambda - \cos^2\varrho_{,,} + 2\cos\lambda\cos\varrho_{,,}\cos\varrho - \cos\varrho^2}\right\} \ldots\ldots (4)$$

This expression is probably not much unlike that which John Bernoulli obtained, allowance being made for the greater generality of its objects and the difference of the notations. As, however, he merely describes it, we can only conjecture what it might have been*: at all events it does not greatly differ from that given by D'Alembert, in the *Encyclopédie Méthodique*, Art. CRÉPUSCULE. Let us now attend to the reduction of the factor within the brackets: and first, cross multiply, and arrange the resulting terms according to the powers of cos ϱ; then divide all the terms by $\cos^2\varrho_{,,} - \cos^2\varrho_{,}$: our result is

$$\cos^4\varrho - 2\cos\lambda\,\frac{1+\cos\varrho_{,}\cos\varrho_{,,}}{\cos\varrho_{,}+\cos\varrho_{,,}}\cos^3\varrho - \sin^2\lambda\cos^2\varrho$$

$$+ 2\cos\lambda\,\frac{1+\cos\varrho_{,}\cos\varrho_{,,}}{\cos\varrho_{,}+\cos\varrho_{,,}}\cos\varrho - \cos^2\lambda;$$

which may again be readily changed into

$$(1-\sin^2\varrho)^2 + 2\cos\lambda\,.\,\frac{1+\cos\varrho_{,}\cos\varrho_{,,}}{\cos\varrho_{,}+\cos\varrho_{,,}}\,.\,\cos\varrho\,(1-\cos^2\varrho)$$

$$- \sin^2\lambda\,(1-\sin^2\varrho) - \cos^2\lambda;$$

and this is at once converted into

$$\sin^2\varrho\left\{\cos^2\varrho - 2\cos\lambda\,\frac{1+\cos\varrho_{,}\cos\varrho_{,,}}{\cos\varrho_{,}+\cos\varrho_{,,}}\cos\varrho + \cos^2\lambda\right\} \ldots\ldots\ldots (5)$$

Inserting instead of the bracketed factor of (4) its value given by (5), we have that equation (4) at once transformed into

$$\cos^2\varrho - 2\cos\lambda\,\frac{1+\cos\varrho_{,}\cos\varrho_{,,}}{\cos\varrho_{,}+\cos\varrho_{,,}}\cos\varrho + \cos^2\lambda = 0 \ldots\ldots (6)$$

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* Johan. Bernoulli, *Opera Omnia*, tome i. p. 64.

We thus see that the factor $\sin^2 \varrho$ which arose out of the process of finding (5) is really a *foreign factor* introduced, as those factors so generally are, by elimination: and we see too how it arose; viz. by Bernoulli and D'Alembert having cancelled the factor $\operatorname{cosec}^2 \varrho$ from equation (4) merely because it was a factor of the whole equation. Had this view occurred to them, they would have depressed their biquadratic to a quadratic at once, and have escaped the embarrassment created by attempting to prove that certain roots contained in it did not fulfill the conditions of the problem, and were *therefore* inadmissible. But an important question here arose—*what is the signification of this result, and how did it make its appearance intermingled with the proper solution of the problem?* It is not enough to take those parts of a result which we can readily interpret, and reject all the others as useless or unmeaning: though such is and always has been so greatly the fashion, that scarcely a single author has insisted upon an unflinching determination to consider every solution incomplete which stops short of this consummation. It will, I have no doubt, ere long, become a fundamental principle in the philosophy of mathematics—that *every part of an algebraical result admits of complete interpretation, either by reference to the conditions which were expressed in the fundamental equations, or else in the hypotheses, tacitly made (in order to apply our transforming operations,) in the various subsequent stages of the solution.* It is, however, so much the custom to discard, without consideration or remark, all such results as do not admit of a ready and obvious application to the immediate objects before us, that facility of interpretation is the rarest of all the faculties of the geometer that we find in any considerable degree developed. No factor ought to be rejected for which a satisfactory reason cannot be given, *nor ought it to be considered foreign till the step at which it was introduced, is distinctly ascertained*: and it bespeaks both a bad taste and unpardonably negligent habits of research, to leave any part of the final equation unexplained, even in the most trivial inquiry which may be undertaken *.

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* The valuable discussion and illustrations of this principle, given by Mr. Babbage in vol. ii. of the Cambridge Philosophical Transactions, cannot be too earnestly recommended to every young mathematician who would form a proper taste in the modern analysis, nor too diligently studied by every one who aspires to a higher character than that of a mere algebraical machine.

With regard, however, to the metaphysical views entertained by that distinguished philosopher, on the relations between the mind and certain objects of mental action, as expressed in that paper, I offer no opinion *here*, as I shall have occasion to moot that subject in another place.

Resolving now the quadratic equation (6), we have

$$\cos \varrho = \cos \lambda \, \frac{1 + \cos \rho_{,} \cos \rho_{,,} \pm \sin \rho_{,} \sin \rho_{,,}}{\cos \rho_{,} + \cos \rho_{,,}}$$

$$= \cos \lambda \, \frac{1 \pm \cos \overline{\rho_{,} \mp \rho_{,,}}}{\cos \rho_{,} + \cos \rho_{,,}}$$

$$= \cos \lambda \, \frac{\cos^2 \frac{1}{2} \overline{\rho_{,} \mp \rho_{,,}}}{\cos \frac{1}{2} \overline{\rho_{,} + \rho_{,,}} \cos \frac{1}{2} \overline{\rho_{,} \backsim \rho_{,,}}} \quad \ldots\ldots\ldots \; (7)$$

But the question now naturally arose in D'Alembert's mind, which of these roots belongs to the *minimum*? or do they *both* designate *minima*?

It is difficult to account for that distinguished geometer overlooking the simple and common test furnished by the differential calculus itself for deciding this question: it is even difficult to conceive how the consideration of the problem itself, as presented by the diagram on the globe, should have failed to point out to him the proper interpretation of the twofold result in our last equation: yet such is the case. He devotes considerable space to proving that the lower sign of our equation does not answer to a minimum twilight. What it does answer to he does not attempt to explain; but he denies that it answers to the maximum.

The general fact, that functions of one independent variable which do not admit of indefinite increase and diminution, can never have two maxima without the intermediate occurrence of a minimum, nor two minima without an intermediate maximum, might have guided him to discover from the figure itself, that the two roots could not both indicate minima; but rather that one of them necessarily indicated the time of *maximum twilight.* However, the common appeal to the second differential coefficient determines it at once. For differentiating (6), and inserting the values of cos ρ given in (7) in the equation, we have for the test the value of

$$\cos \lambda \, \frac{1 + \cos \rho_{,} \cos \rho_{,,}}{\cos \rho_{,} + \cos \rho_{,,}} \mp \cos \lambda \, \frac{\sin \rho_{,} \sin \rho_{,,}}{\cos \rho_{,} + \cos \rho_{,,}}$$

$$- \cos \lambda \, \frac{1 + \cos \rho_{,} \cos \rho_{,,}}{\cos \rho_{,} + \cos \rho_{,,}},$$

the first and third terms of which cancel, and leave, for the test, the sign of

$$\mp \cos \lambda \, \frac{\sin \rho_{,} \sin \rho_{,,}}{\cos \rho_{,} + \cos \rho_{,,}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \; (8)$$

We thus see that the problem has never yet been fully solved, since the determination of the minimum depends not alone upon the choice of the signs + or − in the equation (7), but also upon the values of $\lambda_{,}$ $\rho_{,}$ and $\rho_{,,}$. The way in which this takes place is too obvious to need any further remark.

The interpretation, therefore, of Bernoulli's equation is completed and the problem at the same time resolved in the most general case that can be proposed on a spherical earth, and with a uniform declination for the period of twilight on the day in question. It might hence seem that the inquiry, in reference to everything of value which it can afford, might be properly concluded here. Nevertheless, as the curious properties which Delambre has deduced, both in his *Astronomie* and in his *Histoire de l'Astronomie*, for the particular case he considered, have in several instances remarkable analogies in the more general one above discussed, it will not be out of place to annex a few of them to the preceding investigation.

[To be continued.]

XXXIV. *Theory of the Correction to be applied to a Ball-Pendulum for the Reduction to a Vacuum. By the Rev.* J. Challis, *Fellow of the Cambridge Philosophical Society**.

IN a previous communication respecting the resistance to the motion of small spherical bodies in elastic fluids†, I attempted to explain, entirely from theoretical considerations, the manner inwhich the air is acted upon, when a pendulum consisting of a small spherical ball suspended by a very slender wire, performs very small oscillations in it; but I omitted to enter upon any calculation to ascertain the numerical value of the correction required for reducing the time of vibration in air to that in a vacuum. As the theory there advanced is competent to obtain such a result without the aid of experiment, I propose to make this the object of the present communication.

The following equation was obtained in the paper referred to:—

$$Mv^2 + mv^2 = 2g(M-\mu)(h-x),$$

in which M is the mass of the ball, v the velocity of its centre, μ the mass of an equal volume of air, g the force of gravity, $h-x$ the vertical descent of the centre of the ball. The equation without the term mv^2, is that which was formerly employed in reducing to a vacuum, the effect of the motion of the fluid not being taken into account. According to our theory $m v^2$ is to be added, in consequence of the simultaneous movement of the air with the pendulum. The reasoning led to the conclusion that so little change of density takes place at the surface of the ball, that we may consider the air to be put in motion just as if it were incompressible. The influence of the air carried by the suspending wire was neglected in the theory. Also the surface of the ball was supposed to be perfectly smooth, so as to impress no motion on the aërial particles in the direction of a tangent plane. Hence the air in contact with the ball will move in directions normal to its surface, and consequently directed to a centre. Because the density is very nearly unchanged, the velocity at a given instant will very nearly vary at different points, in a radius produced, inversely as the square of the distance from the centre.

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* Communicated by the Author.

† See Lond. and Edinb. Phil. Mag. vol. i. p. 40.

These results being admitted, we may proceed to calculate $m v^2$. For conceive two straight lines to be drawn at any instant through the centre of the ball, one in the direction of its motion, the other in any direction making an angle θ with the first. Let the plane of these two lines make an angle ϕ with a plane through the centre of the ball, at right angles to the suspending wire. The velocity of the air at the point where the latter line meets the surface is $v \cos \theta$; and at any point P on the line, distant by r from the centre, the velocity is $\frac{v b^2}{r^2} \cos \theta$, b being the radius of the ball. The mass of a fluid element at P, its density being 1, is

$$d r \times r d \theta \times r \sin \theta \, d \phi,$$

and the *vis viva* of the fluid in motion, or $m v^2$, is equal to

$$\iiint \left(\frac{v b^2}{r^2} \cos \theta \right)^2 r^2 \sin \theta \, d r \, d \theta \, d \varphi.$$

The integral with respect to ϕ is to be taken from 0 to 2π, with respect to θ from 0 to π, and with respect to r from b to infinity. Hence

$$m v^2 = b^4 v^2 \iiint \frac{d r}{r^2} \cos^2 \theta \sin \theta \, d \theta \, d \varphi$$

$$= 2 \pi b^4 v^2 \iint \frac{d r}{r^2} \cos^2 \theta \sin \theta \, d \theta$$

$$= \frac{4 \pi b^4 v^2}{3} \int \frac{d r}{r^2} = \frac{4 \pi b^3 v^2}{3}.$$

It follows that $m = \frac{4\pi b^3}{3} = \mu$, and that

$$M v^2 + \mu v^2 = 2g\,(M-\mu)\,(h-x).$$

Hence $-\frac{v\,dv}{dx}$ or $f = g\,\frac{M-\mu}{M+\mu} = g\left(1-\frac{2\mu}{M}\right)$ very nearly.

and if l' = the length of the seconds pendulum in air, l in vacuum,

$$\frac{l'}{l} = \frac{f}{g} = 1 - \frac{2\mu}{M}, \text{ and } l' = l\left(1-\frac{2\mu}{M}\right).$$

The coefficient by which the old correction is to be multiplied, is consequently 2.

M. Bessel has obtained by experiment 1·956 for the value of this coefficient. The experiments of Mr. Baily (Transactions of Royal Society for 1832, p. 399*,) give 1·864 for spheres 1½ inch in diameter, and 1·748 for spheres 2 inches in diameter, and consequently show that the coefficient is different for spheres of different diameter. No such difference is recognised either by the present theory, or by that of M. Poisson, who has taken into account the effect of the friction of the air against the surface of the ball. The theory of this difference, therefore, stands in need of further inquiry. Experiment is the most proper means of determining the *amount* of correction to be applied to the pendulum, but theory combined with it may enable us to determine the causes to which the correction is due.

Papworth St. Everard, Aug. 2, 1833.

XXXV. *Characters of some undescribed Genera and Species of* Araneidæ. *By* John Blackwall, *Esq. F.L.S. &c.*†

Tribe, Inequitelæ, Latreille.

Genus, *Neriene.*

EYES eight in number, unequal in size, disposed in two transverse rows on the anterior part of the cephalothorax; the intermediate eyes of both rows form a trapezoid whose anterior side is considerably the shortest; the lateral ones are placed obliquely, in pairs, each pair being seated on a small eminence and geminated; the posterior eyes of the trapezoid are larger, and the anterior ones much smaller than the rest.

Maxillæ strong, inclined towards the lip, slightly dilated at the base where the palpi are inserted, and greatly so at the apex, which is obliquely truncated externally.

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* An abstract of Mr. Baily's paper will be found in Lond. and Edinb. Phil. Mag. vol. i. p. 379.—Edit.

† Communicated by the Author; by whom other undescribed *Araneidæ* were characterized in our last Number.

Lip short, broad, prominent at the apex, and semicircular.

Legs moderately long and robust; the anterior and posterior pairs, which are the longest, equal in length; the third pair is the shortest.

The spiders of this genus fabricate small horizontal sheets of web of a slight texture, among coarse herbage, or in cavities beneath stones, on the under side of which they take their station, in an inverted position, and watch for their prey.

Neriene marginata.

Cephalothorax oval and glossy; the anterior part, on which the eyes are seated, elevated but obtuse; the posterior part depressed, with an indentation in the medial line. Mandibles strong, armed with teeth on the inner surface, and inclined towards the pectus which is heart-shaped. Maxillæ enlarged at their apices, and slightly inclined towards the lip. These parts are of a very dark brownish-black colour. Legs and palpi provided with erect spines; their colour is reddish-brown, with bands of a darker hue. Both of the superior tarsal claws are pectinated, and the inferior one is inflected near its base. A plain claw, slightly curved, terminates each palpus. Abdomen oval, rather convex above, projecting over the base of the cephalothorax; the upper part is brownish-black bordered by a dentated band of pale brown thickly spotted with white, which passes above the spinners, but whose continuity is interrupted in front by a black streak intersecting it at right angles; a series of curved, angular lines of a pale-brown colour, minutely spotted with white, extends along the middle; their convexity is towards each other, and their apices are directed forwards. Sides and under part of the abdomen brownish-black, marked with a few white spots, four minute ones, describing a large quadrangle, occurring on the latter. Plates of the spiracles dark red-brown.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{3}{20}$ths of an inch; length of the cephalothorax $\frac{1}{14}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{15}$; length of an anterior leg $\frac{1}{3}$; length of a leg of the third pair $\frac{1}{6}$.

The male resembles the female in colour, and in the relative length of its legs, but the absolute length of those organs is greater, an anterior one measuring $\frac{1}{4}$th of an inch. The third and fourth joints of the palpi are short, the latter, which is much the stronger, being fringed with long bristles on the outer side of the upper part; the fifth joint is oval, convex externally, concave within, comprising the sexual organs; they are highly developed, complex with spiny processes, and are of a brownish-black colour tinged with red. Very old males are darker coloured and have red legs.

In the months of October and November this spider is common in the plantations about Crumpsall Hall, constructing its web among the grass under the trees.

Neriene rubens.

Cephalothorax inversely heart-shaped, slightly convex above, and glossy, with a depression and an indentation in the posterior region; in front is an abrupt eminence, on which are the eyes, placed on black spots. Mandibles robust, conical, strongly toothed on the inner surface, and inclined towards the pectus, which is heart-shaped. Maxillæ moderately enlarged at their apices, and inclined towards the lip. Legs and palpi hairy, the former provided with a few spines; the two superior tarsal claws are pectinated, and the inferior one is inflected near its base. Palpi abundantly supplied with black spines, one longer than the rest, projecting from the anterior extremity of the third joint. These parts, with the exceptions already noticed, are of a yellowish-red colour. Abdomen oval, somewhat convex above, projecting over the base of the cephalothorax, thinly clad with hair, glossy, and of a red-brown hue, which varies in intensity in different individuals. Sexual organs black with a tinge of red. Plates of the spiracles pale orange.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{8}$th of an inch; length of the cephalothorax $\frac{1}{16}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{18}$; length of an anterior leg $\frac{1}{6}$.

The male is rather smaller than the female, but the relative length of its legs is the same; the tibiæ of the first and second pairs are dilated underneath at their anterior extremities, and these enlargements are thickly clad with fine long hairs. The second joint of the palpi is very powerful, enlarging gradually from the base to its anterior extremity, which is armed with a strong spur, and a great number of minute, sharp-pointed, black spines, on the upper side; the third joint is short and robust; the fourth is furnished with two apophyses; the inner one is much the longer and tapers to a point: it is curved obliquely across the upper part of the fifth joint, and is provided with a strong projecting point near its base; the exterior apophysis is much shorter and more obtuse: the fifth joint is oval, convex externally, concave within, comprising the sexual organs, which are complicated in structure, exhibiting several curved, spiny processes, and are of a dark red-brown colour.

Mr. T. Blackwall found this species among heath, in Trafford Park, near Manchester, in October 1832, at which season of the year it pairs.

Neriene cornuta.

Cephalothorax glossy, inversely heart-shaped, depressed behind, with a large indentation in the medial line, and the sides are marked with furrows extending from the superior part to the margins; the anterior part is elevated and surmounted by two conical protuberances directed forwards, below which, in front, the eyes are situated. Mandibles strong, conical, perpendicular, and armed with teeth on the inner surface. Maxillæ inclined towards the lip, which they encompass. Lip short, prominent at the apex, and semicircular. Pectus convex and heart-shaped. These parts are of a very dark-brown colour, the anterior portion of the cephalothorax being almost black. The colour of the legs and palpi is light red-brown. The two superior tarsal claws are slightly pectinated, and the inferior one is inflected near its base. The fourth joint of the palpi is shorter than the third, and projects three apophyses from its anterior extremity, one on the inner side, which is long, curved, and pointed; a small one in front, which is also curved, and pointed; and one on the under side, which is short and obtuse; the fifth joint is oval, convex externally, concave within, comprising the sexual organs; they are highly developed, complex with spiny processes, and are of a dark red-brown colour. Abdomen oval, rather convex above, projecting over the base of the cephalothorax, sparingly supplied with short hairs, glossy, and black, with the exception of the plates of the spiracles, which are of a pale yellow colour.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{10}$th of an inch; length of the cephalothorax $\frac{1}{20}$; breadth $\frac{1}{22}$; breadth of the abdomen $\frac{1}{21}$; length of an anterior leg $\frac{3}{20}$; length of a leg of the third pair $\frac{1}{9}$.

I captured males of this species in May 1833, on rails and gates at Oakland; but the female has hitherto escaped detection.

Closely allied to the spiders of the genus *Linyphia* by the disposition and relative size of their eyes, but approximating more nearly to those of the genus *Theridion* in the structure of the mouth and the relative length of their legs, the *Nerienæ* present another striking instance of propinquity between the *Inequitelæ* and *Orbitelæ*; a connexion which is further established by the mixed character of their œconomy.

Tribe, ORBITELÆ, } Latreille.
Genus, LINYPHIA, }

Linyphia minuta.

Cephalothorax glossy, inversely heart-shaped, prominent before, with an indentation in the medial line of the posterior region, and of a dark-brown colour. Pectus heart-shaped, and very dark-brown, approaching to black. Mandibles strong, vertical, and of a dark red-brown hue. Maxillæ straight, nearly quadrate, having the exterior angle at the apex curvilinear. Lip short, and semicircular. The colour of these organs is the same as that of the mandibles. Legs and palpi long, slender, and supplied with numerous delicate spines; they are reddish-brown with brownish-black bands. First pair of legs the longest, then the second, the third pair being the shortest; the two superior tarsal claws are pectinated, and the inferior one is inflected near its base. Each palpus is terminated by a single claw, slightly curved and pectinated. The four intermediate eyes form a trapezoid whose shortest side is in front; the other four are disposed in pairs on the sides of the trapezoid, the eyes constituting each pair being contiguous, and placed obliquely on an eminence; the two posterior eyes of the trapezoid are the largest, and the two anterior ones the smallest of the eight. Abdomen oval, convex above, projecting over the base of the cephalothorax, and thinly clad with hair; the upper part is pale-brown minutely spotted with yellowish-white; along the middle extends a series of strongly marked, brownish-black angular lines with their vertices directed forwards, and a little above the spinners is an irregular, transverse, semicircular line of a yellowish-white colour: the sides are brownish-black with a slightly curved line of yellowish-white, extending from the anterior part rather more than half-way towards the spinners; underneath, the abdomen is of a brownish-black colour, and the plates of the spiracles are yellowish-white.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{6}$th of an inch; length of the cephalothorax $\frac{1}{14}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{13}$; length of an anterior leg $\frac{3}{10}$.

The body of the male is smaller and more slender than that of the female, but the legs are longer, an anterior one measuring $\frac{7}{20}$ths of an inch. The third and fourth joints of the palpi are short and strong; the fifth has two conical processes, or apophyses, on the upper part, near its articulation with the fourth joint; it is convex externally, concave within, comprising the sexual organs, which are of a red-brown colour.

I first observed this spider in little frequented rooms in Crumpsall Hall; it does not confine itself, however, to the interior of buildings, but may frequently be found on trees and shrubs, particularly such as grow against walls. In the month of September the female deposits thirty or forty spherical eggs, of a yellowish-white colour, in a cocoon of white silk, of a slight texture, and subglobose form, measuring about $\frac{1}{5}$th of an inch in diameter.

Linyphia luteola.

This spider resembles *Linyphia minuta* in the form of the cephalothorax, except that the anterior part, on which the eyes are seated, is more prominent and acute; it is glabrous, and of a pale yellowish-brown colour, with black margins and a slender band of the same hue along the medial line. The eyes are placed on black spots, their arrangement and relative size being the same as in *Linyphia minuta.* The parallelism between the two species holds good also as regards the structure of the mandibles, the maxillæ, the lip, and the pectus. These parts are of the same colour as the ground of the cephalothorax. The legs are long and slender, the palpi rather short; both are provided with claws and delicate spines similar in structure to those of *Linyphia minuta*; their colour is a uniform pale yellowish-brown. The first pair of legs is the longest, then the second, the third pair being the shortest. Abdomen oval, compressed, remarkably convex above, projecting over the base of the cephalothorax; its colour is pale yellow, with minute white spots on the upper part; the sides are obscurely marked with oblique lines of a blackish hue, and above the spinners are several of a similar tint and an angular form. Plates of the spiracles yellow. In some individuals scarcely a trace of the black lines can be perceived.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{5}$th of an inch; length of the cephalothorax $\frac{1}{12}$; breadth $\frac{1}{16}$; breadth of the abdomen $\frac{1}{10}$; length of an anterior leg $\frac{2}{5}$.

The male is smaller and more slender than the female, but the cephalothorax is longer, measuring $\frac{1}{10}$th of an inch; it is very prominent and acute before, and is furnished with numerous strong, black bristles, particularly at the apex. The legs also are more elongated, an anterior one measuring $\frac{9}{20}$ths of an inch. The maxillæ are exceedingly convex externally, immediately above the insertion of the palpi; the second joint of the latter organs is robust; the third and fourth joints are very short, a strong bristle, rough with projecting points on the under side, depending from a prominence on the upper part of the anterior extremity of the former; the fifth joint is oval, convex externally, concave within, comprising the sexual organs, which are complex, with a curved pointed projection on the upper part; their colour is red-brown.

This species of *Linyphia* is plentiful in September and October, in the plantations about Crumpsall Hall, constructing, among the coarse grass beneath the trees, a horizontal web, three or four inches in diameter, on a plan similar to that employed by the other species of the genus. Like them, too, it is usually found on the under side of the horizontal sheet in an inverted position.

Tribe, CITIGRADÆ, Latreille.

Genus, *Hecaërge*.

Eyes eight in number, unequal in size, disposed in two transverse rows on the anterior part of the cephalothorax; the four constituting the anterior row, which is slightly curved backwards, are adjacent and minute, the two lateral ones being the smallest of the eight; the posterior row is greatly curved, with its convexity directed forwards; it comprises the other four eyes, which are large and separated by moderately wide intervals.

Maxillæ short, strong, convex on the under side, enlarged at the base where the palpi are inserted, rounded at the extremities, and inclined towards the lip.

Lip small, as broad as long, triangular, truncated at the apex.

Legs powerful; the fourth pair is the longest, then the first, the third pair being the shortest.

The spiders belonging to this genus are erratic, taking their prey by surprise.

Hecaërge maculata.

Cephalothorax oval, convex above, depressed in the posterior region, pointed before, and covered with short hairs; its colour pale yellowish-brown, with a broad dark-brown band extending along each side of the medial line, and two fine lines of the same hue on the lateral margins; a few longish white hairs, directed forwards, occupy the space between the two intermediate pairs of eyes, and immediately below the anterior row of eyes is a small transverse parallel line of a dark red-brown colour. Mandibles small, conical, perpendicular, armed with a curved red nail at the extremity, and a few minute teeth on the inner surface; they are of a pale yellowish-brown colour, with a dark-brown elongated spot in front of each, extending from the base towards the extremity; this spot is palest in the medial line. Maxillæ pale yellowish-brown. Lip dark-brown bordered with pale-brown. Pectus heart-shaped, of a pale-yellow colour faintly tinged with green; eight dark-brown spots occur on its margins; one, which is very minute, and in some individuals wanting altogether, is situated opposite the lip; three are disposed on each side, and one is seated on its posterior extremity. Legs hairy, furnished with strong spines; thighs and terminal joint of the tarsi pale yellowish-brown, the former having two or three longitudinal lines of a dark-brown colour on the upper and outer sides, which are most conspicuous on the first and second pairs, and some minute spots of the same tint underneath; tibiæ and superior tarsal joint dark-brown, those of the first and second pairs of legs having a series of long, moveable, sessile spines on each side of the inferior part. The two tarsal claws are long, curved, and provided with three minute teeth, the terminal one being the longest; beneath them is a small brush, which enables the spider to ascend with facility smooth perpendicular surfaces. Palpi pale yellowish-brown, furnished with a few spines, two of which, on the under side of the fifth joint, are opposite and erect; a long curved claw, having three exceedingly small teeth underneath, terminates each palpus. Abdomen thickly covered with hair, oval, convex above, projecting over the base of the cephalothorax; upper part pale yellowish-brown mingled with white; three small tufts of white hair occur in front, near the cephalothorax; two narrow, obscure, brownish-black streaks, one on each side of the medial line, extend from the anterior part of the abdomen almost a third of its length, and are followed by a series of brownish-black spots, occupying the remainder of the medial line to the spinners, on each side of which is a longitudinal row of very small spots of the same hue; these streaks and spots are comprised between two irregular brownish-black bands composed principally of spots, confluent or nearly so, diminishing in size as they approach the spinners; sides and under part of the abdomen pale yellowish-brown spotted with brownish-black, the spots on the latter minute. Plates of the spiracles yellowish-brown with pale inner margins.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{4}$th of an inch; length of the cephalothorax $\frac{1}{10}$; breadth $\frac{1}{12}$; breadth of the abdomen $\frac{1}{10}$; length of a posterior leg $\frac{3}{8}$; length of a leg of the third pair $\frac{1}{4}$.

The male though smaller than the female is similar to it in colour, and in the relative length of its legs. The third and fourth joints of the palpi are short, a small pointed apophysis projecting from the outer side of the anterior extremity of the latter; the fifth joint is oval, convex above, concave beneath, comprising the sexual organs, which are highly developed, with a curved spiny process extending to the termination of the joint, and are of a red-brown colour.

This species, which I discovered in the woods about Oakland, in Denbighshire, in the month of April 1833, approximates most nearly in its general structure to the spiders of the genera *Lycosa* and *Dolomedes*; by the organization of the feet, however, it appears to connect the *Citigradæ* with the *Laterigradæ*. In the month of June the female constructs a lenticular cocoon of white silk, of a slight texture, measuring about $\frac{3}{10}$ths of an inch in diameter, which she usually attaches to the under side of a stone, depositing in it between twenty and thirty spherical eggs of a yellowish-white colour, not agglutinated together.

Tribe, TUBITELÆ, Latreille.

Genus, *Erigone*, Savigny.

Erigone atra.

Cephalothorax inversely heart-shaped, very prominent before, with an indentation in the medial line of the posterior region. Mandibles powerful, rather convex in front, strongly toothed on the inner surface, and slightly inclined towards the pectus, which is heart-shaped. Maxillæ greatly enlarged at the base where the palpi are inserted, encompassing the lip, which is short and semicircular. Fourth pair of legs the longest, then the first, the third pair being the shortest. The two superior tarsal claws are pectinated, and the inferior one is inflected near its base. Eyes disposed in two transverse rows on the summit of the anterior convexity of the cephalothorax; the intermediate ones of both rows form a square, and the other four are placed obliquely in pairs, one on each side of the square. Abdomen oval, convex above, projecting over the base of the cephalothorax. This spider is sparingly clad with hair, and glossy black, with the exception of the legs and palpi, which, in adults, are of a reddish-brown colour, and of the plates of the spiracles, which are yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{9}$th of an inch; breadth of the abdomen $\frac{1}{20}$; length of a posterior leg $\frac{1}{8}$.

The male differs from the female in several remarkable particulars; it is smaller, and presents, exclusively, some of those peculiarities of structure on which the generic character is established. The lateral margins of the cephalothorax are strongly dentated, and a series of short sharp spines, slightly curved forwards, occupies the medial line of its anterior convexity; each mandible also has a longitudinal row of spines, curved downwards, on its exterior side. (Small spines occur sometimes on the mandibles of old females.) The second joint of the palpi is greatly elongated, much curved, and is armed with strong spines beneath; the third and fourth joints are dilated at their anterior extremities, the former having a large conical apophysis on the lower side, and the latter two terminal apophyses, one short and broad projecting in front, the other more elongated and acute situated underneath; the fifth joint is oblong-oval, convex externally, concave within, comprising the sexual organs, which are highly developed, and of a very dark red-brown colour. The relative length of the legs likewise is different in this sex; the first pair is the longest, measuring $\frac{1}{7}$th of an inch, and the fourth, second, and third pairs decrease successively in longitudinal extent. These organs and the palpi are much redder than those of the female.

Independently of colour, the spider described above may be distinguished by its external structure from the *Erigone vagans* of M. Savigny. (*Description de l'Egypte, seconde édition*, tome xxii. p. 319—321; *Atlas de Zool.*; *Arachn.* pl. i. fig. 9.) The male of the latter species has three rows of spines on the anterior convexity of the cephalothorax; a single row on the under part of each thigh of the first pair of legs, and the second pair of legs is longer than the fourth; whereas the new spider has only one row of spines in the former situation, and none in the latter, a few very minute ones merely occurring on the inner side of the thighs of the fore legs, near their base, and the fourth pair of legs is longer than the second; moreover, each palpus of the female of *Erigone vagans* has a pectinated claw at its extremity, but the palpi of *Erigone atra* are destitute of claws.

This diminutive spider is very plentiful and decidedly diurnal. Endowed with an instinctive impulse to ascend into the atmosphere, it frequently takes aërial excursions, and is active even in winter, when the weather is mild. It pairs in June, and deposits its eggs, which are large in proportion to the size of the animal, six or seven in number, spherical, not agglutinated together, and of a pale yellowish-white colour, in a cocoon of white silk, of a slight texture, and subglobose form, measuring about $\frac{1}{10}$th of an inch in diameter; it is usually placed under stones and in crevices of the earth.

There must be something very remarkable in the internal as well as external organization of this extraordinary spider, for numerous specimens of both sexes submerged in cold water on the 21st of October 1832, remained in that situation till the 22nd of November, an interval of 768 hours, without having their vital energies suspended. It is evident, therefore, that this species possesses the power of abstracting respirable air from water, for though in the act of submersion the spiracles are usually enveloped in a bubble of air, yet so small a supply must soon be exhausted, and, indeed, it speedily disappears.

Oakland, near Llanwrst, Denbighshire,
July 29, 1833.

XXXVI. *Additional Note on Conical Refraction.* *By* J. MacCullagh, *F.T.C.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE introductory part of my note which appeared in your last Number was written in haste, and I have reason to think it may not be rightly understood. You will therefore allow me to add a few observations that seem to be wanting.

The principal thing pointed out in the paper that I published some time ago in the Transactions of the Royal Irish Academy, is a very simple relation between the tangent planes of Fresnel's wave surface and the sections of two reciprocal ellipsoids. Now this relation depends upon the *axes* of the sections, and therefore naturally suggested to me the peculiar cases of circular section in which every diameter is an axis. Thus a new inquiry was opened to my mind. And accordingly, without caring just then to obtain final results, which seemed to be an easy matter at any time, I expressed in conversation my intention of returning to the subject of Fresnel's theory, in a supplementary paper. The design was interrupted, and I was prevented from attending to it again, until I was told that Professor Hamilton had discovered cusps and circles of contact on the wave surface. This reminded me of the cases of circular section, and the details given in my last note were immediately deduced.

I am, Gentlemen, &c.

J. MacCullagh.

Trinity College, Dublin, August 16, 1833.

XXXVII. *A Catalogue of Comets. By the Rev.* T. J. Hussey, *A.M. Rector of Hayes, Kent.*

[Continued from p. 102.]

[The Chronology employed is that of Petau or Petavius.]

A, the comet of 1680. B, that of 1652. C (Halley's), that of 1682. D, that of 1759. E, that of 1661. F, that of 1677.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
171	673	...	......	......	Vita S^{t}. Leod. Centur. Chron. &c.	
172	676	...	January	Virgo...........	Chinese Records.	
173	—	...	Aug. Sept......	Gem., Ur. Maj.	Anast. Paul. Diac. Chin. Records.	Seen 3 months.
174	681	...	Oct. Novemb.	Hercul. Aquila	Chinese Records.	Seen 17 days.
175	683	...	April, May	Auriga, Taurus	Chinese Records.	
176	684	...	Sept. Oct......	Tow^{ds} the West	Chinese Records.	
177	—	B	December	Taurus	Anast. Paul. Diac. &c.	
178	707	...	November.....	Tow^{ds} the West	Chinese Records.	
179	708	...	March	Musca, Taurus	Chinese Records.	
180	—	...	September	......	Chinese Records.	
181	711	...	......	......	Italy in Centil. Ptolem.	Continued 11 nights with a visible motion.
182	712	...	August.........	C. Beren., Leo, Virgo, Bootes	Chinese Records.	
183	716	...	......	......	Sabell. Bizar. &c.	
184	729	F?	......	......	Bede. Anast. &c.	
185	730	...	Aug. Sept......	Auriga, Taurus	Chinese Records.	
186	738	...	April..........	Ursa Major...	Chinese Records.	
187	744	...	......	......	Theoph. Cedren.	
188	760	...	May............	Taurus, Gemini, &c. ...	Chinese Records.	Seen 59 days.
189	—	...	May, June.....	To the West	Chinese Records.	
190	762	...	......	......	Theophan. &c.	
191	767	...	January........	Delphinus......	Chinese Records.	
192	770	...	May	Auriga, Taurus	Chinese Records.	
193	—	...	June, July	Auriga, Camel.	Chinese Records.	
194	773	..	January........	Orion	Chinese Records.	
195	813	...	August.........	......	Theoph. Sim. Logoth. Zonar, &c.	
196	815	...	April	Leo	Chinese Records.	
197	817	...	February	Sagittarius.....	Chi. Rec. Eginh. Aimoin, &c.	
198	821	...	February	Crater, Leo.		

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
199	—	...	July	Taurus.		
200	837	...	......	......	Chinese Records.	Elements computed by Pingré*.
201	—	...	September....	Aquar. Equul. Pegasus....	Chi. Rec. Boeth.	
202	838	...	November....	Corvus.........	Chinese Records.	
203	—	...	Nov. Dec.....	Sagitt. Scorpio	Chinese Records.	
204	839	...	February......	Aquarius......	Chin. Rec. Annal. Bertin.	
205	—	...	March, April..	Perseus......	Chin. Rec. Annal. Bertin.	
206	840	...	March, April..	Pega. Androm.	Chinese Records.	
207	—	...	December......	In the East...	Chinese Records.	
208	841	...	June, July.....	Sagitt. Aquar.	Alber. Casin. Chin. Rec.	
209	—	...	December to February 842	Pisces, Pegas.	Chinese Records. Nith. Hist.	
210	852	...	March.........	Orion.......	Chinese Records.	
211	855	C	......	......	Chron. Mallcac.	Seen during 20 nights.
212	857	...	September....	Scorpio.......	Chinese Records.	
213	858	...	April...........	In the East ..	Ptolem. Hist. Martin. Fuld.	
214	864	...	June...........	Aries.........	Chron. Floriac. Chinese Rec.	
215	866	...	April...........	......	Const. Porphyr.	
216	868	...	January........	Aries, Musca..	Chinese Records. Ann. Fuld. &c.	Seen for about 17 nights.
217	869	...	September....	Pers. Cap. Med.	Chi. Ann. Pontan.	Direction north-east.
218	873	...	......	....	Chron. Andega.	
219	875	...	June...........	Aries...........	Ann. Fuld. Chro. Casauv. &c.	
220	877	...	March	Libra...........	Chron. Novalic.	Seen 15 nights.
221	882	...	January......	....	Annal. Fuld. Aventin. &c.	
222	885	...	......	Betw^{n} Perseus and Gemini	Chinese Records.	
223	886	...	June...........	Scorp. Sagitt. U. Ma. Bootes.	Chinese Records.	

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* Passage through the perihelion in mean time at Greenwich: March $0^{d}\ 23^{h}\ 51^{m}\ 0^{s}$.—Long. of the perihel. on the orbit of the comet, $9^{s}\ 19°\ 3'\ 0''$.—Long. of the ascending node, $6^{s}\ 26°\ 33'\ 0''$.—Angle between the perihel. and the node, $9^{s}\ 7°\ 30'\ 0''$.—Inclination of the orbit, 10° or 12°.—Perihel. distance, 0·580000.—Log. of the mean motion, 0·314986.—Direction R.

The angle between the perihelion and the node having been left out of the elements of the four comets published in the last Number, the omission is here supplied.

No.	Year.			
50	240	2^{s}	22′	0′
135	539	8 2	15	30
139	565	2	10	0
140	—	2	19	30

[To be continued.]

XXXVIII. *Contributions to the Geology of Northumberland and Durham.* *By* N. J. Winch, *Esq. F.G.S. & A.L.S.*

[Continued from p. 99.]

An Account of Strata sunk through in the Blossom Pit, Hetton Colliery, begun December 19, 1820, *Durham.*

	F.	Y.	F.	I.	F.	Y.	F.	I.
Outset above the surface	1	0	0	0				
Soil	0	0	2	0				
Sand	1	1	0	0				
Gravel	0	1	1	0				
Limestone marl and soft limestone ...	3	0	0	0				
Limestone of various kinds, and the water increased gradually, from about the depth of 16 fathoms (where the machine began to pump,) to the bottom of it, to nearly 2000 gallons per minute; viz.								
—— Yellow limestone in different beds	15							
—— Blue ditto and flaggy near the bottom (called blue rag)	7							
Blue metal	0							
Sandstone when broken into was drawn by the pumps	0							
White metal stone, very soft	0							
Gray scamy metal stone	0							
Strong brown limestone mixed with whin	1							
Blue gray metal mixed with water and very soft	5							
Coal mixed with black metal	0							
					38	1	1	4
Gray metal stone								
Blue metal								
Coal mixed with black metal								
					2	1	0	5
Gray metal stone and water		1	2	10				

Section of the 4-feet Coal Seam.

	Y.	Ft.	In.	F.	Y.	F.	I.	F.	Y.	F.	I.
Coal	0	1	0								
Gray metal	0	0	4								
Black ditto with *coal*	0	0	7								
Coal	0	1	10	0	1	0	9				
								3	1	0	7
Gray metal stone or thill					0	1	4				
Gray ditto with partings					0	0	10				
Strong white post, and water in it, near the bottom, and spongy					0	1	8				
Three-quarter *coal* seam and water in it ...					0	2	4				
								8	0	0	2
Gray metal stone or thill						1					
White post											
Gray metal											
White post with partings and water											
Carried forward				8	1	0	5	2	1	2	6

				Fa.	*Yds.*	*Ft.*	*In.*	*Fa.*	*Yds.*	*Ft.*	*In.*
Brought forward ...				8	1	0	5	52	1	2	6
Gray metal stone				0	0	2	8				
Coal mixed with black metal				0	0	1	6				
								9	0	1	7
Thill				0	0	0	8				
White post				0	0	2	5				
Gray metal				0	0	2	2				
Gray metal stone with post girdles				0	0	1	4				
Gray metal				0	0	0	6				
Coal				0	0	0	10				
								1	0	1	11
Thill				0	0	0	3				
Gray metal stone				0	0	0	10				
White post with wet partings 8 feet down ...				1	1	1	2				
Gray metal stone mixed with post				0	0	2	9				
Black and blue metal				1	1	2	10				
Coal				0	0	0	3				
								4	0	2	1
Gray metal stone				0	0	1	3				
White post				1	0	1	9				
Gray and white post mixed with gray metal stone				1	1	2	2				
Whin				0	1	0	7				
White post and partings of blue metal and water wedged off				0	0	2	10				
Strong white post with a parting at bottom and water				1	1	1	6				
Strong white post and spongy with partings and water				4	1	2	11				
Gray and white post and the water wedged off				3	0	1	3				
Gray metal stone				0	1	1	6				
Coal	0 y.	0 ft.	4 in.								
Gray metal	0	1	0								
Coal	0	1	3								
Band	0	0	5								
Coal	1	0	2								
Band	0	0	4								
Coal mixed with black metal ...	0	0	11	1	0	1	5				
								16	0	2	2
Thill				0	0	1	8				
Gray metal stone with large balls of iron-stone				1	0	2	4				
Strong gray metal with girdles				3	1	0	9				
Black metal with scares of coal				0	0	0	9				
Coal				0	0	1	2				
								5	1	0	8
Black gray metal with scares of coal				0	0	2	8				
Dark gray metal with balls of ironstone ...				0	0	2	6				
Strong gray metal stone with strong dunnish post girdles				3	0	1	10				
Gray metal				1	0	0	8				
Five quarter *coal* seam				0	0	1	7				
								5	1	0	3
Carried forward								94	4	2	11

	Fa.	Yds.	Ft.	In.	Fa.	Yds.	Ft.	In.
Brought forward ...					94	4	2	11
Black metal with scares of coal	0	0	0	6				
Dark gray metal	0	1	1	4				
Coal	0	0	0	5				
Gray metal stone with post girdles	5	1	1	6				
Gray post	0	1	1	10				
Blue and gray metal stone	3	1	2	9				
Do. . do. with strong girdles	1	1	2	3				
Section of the High main coal seam, on the Wear, or Yard coal seam, on the Tyne.								
Top *Coal* ... ... 0y. 1ft. 8in.								
Band 0 0 1								
Coal 1 1 9								
	1	0	0	6	14	0	2	1
Total depth to the Main coal seam sunk through 3rd September 1822					109	1	1	3

An Admeasurement of the Minor Pit, (which is 45 *yards to the West of the Blossom Pit), to the Main Coal Seam and Strata sunk through from there to the Hutton Coal Seam, begun December* 23, 1820.

	Fa.	Yds.	Ft.	In.	Fa.	Yds.	Ft.	In.
Total depth of the Minor Pit to the thill of the Main Coal Seam, sunk through various strata similar to those specified in the Blossom Pit sinking account, as above	....				108	1	1	1
White post	0	0	0	8				
Gray and white post	1	1	2	0				
Black metal stone	0	0	1	4				
Blue metal with iron stone girdles	1	0	2	4				
Black metal	0	0	1	4				
Gray metal stone	0	0	2	10				
Coal	0	0	0	5				
					4	0	1	11
Strong gray metal stone	0	0	2	0				
Gray and white post	4	1	0	9				
Gray metal	1	1	0	2				
Coal	0	0	1	2				
					6	1	1	1
Gray metal	0	1	0	9				
Coal	0	0	0	4				
					0	1	1	1
Gray metal	0	1	1	2				
Blue metal	0	1	1	0				
Gray metal	1	1	2	6				
Blue metal with coalpipes	0	1	0	8				
Strong gray post	0	1	2	8				
Do. do. with strong girdles	0	1	2	2				
Whin	0	1	0	0				
Soft gray metal	2	1	2	8				
Carried forward	9	0	0	10	120	0	2	2

				Fa.	Yds.	Ft.	In.	Fa.	Yds.	Ft.	In.
Brought forward				9	0	0	10	120	0	2	2
Low main *coal* seam, on the Wear, or Five-quarter coal seam, on the Tyne				0	1	1	1				
								9	1	1	11
White post				2	0	0	6				
Whin				0	0	2	2				
Gray metal with post girdles				2	0	0	10				
Black and blue metal stone				1	0	0	0				
Coal				0	0	1	0				
								5	1	1	6
Gray metal with post girdles				0	1	2	11				
Coal	0y.	0ft.	4in								
Band	0	0	1¾								
Coal	0	0	7								
Thill	1	1	0								
				0	1	2	0¾				
Strong gray metal				1	0	2	0				
Gray post				1	0	2	4				
Black metal				0	1	0	0				
Blue metal with post girdles				0	0	1	4				
Strong white post				4	0	0	0				
Gray metal and post girdles				1	0	1	0				
Section of the Hutton coal seam, on the Wear, or Low main coal seam, on the Tyne.											
Coal	1y.	1ft.	7½in								
Band	0	0	3½								
Bottom *Coal* (coarse)	0	1	3								
				1	0	0	2				
								11	0	2	9¾
Total depth to the Hutton seam, or Low main seam of the collieries on the Tyne				...	...			147	0	2	4¾

The colliery, or rather the three collieries, in the neighbourhood of Hetton in the county of Durham, is at present the most important mining concern in this coal-field. These pits are said to be capable of working two hundred thousand Newcastle chaldrons of coals annually: they are situated about seven miles from Sunderland, to the south-west, and the shafts are sunk through the magnesian limestone, which here covers the coal-measures. The following extract of a letter from a professional man, on the subject of these mines, is worthy of insertion.

" A bed of soft sandstone* was found under the magnesian limestone, abounding with water, in the Hetton and Elemore Pits; but at Eppleton sinking, no water was met with till near the bottom of the sandstone. It may be remarked that at the Hetton Pits there is twenty-six fathoms of limestone, and only four feet four inches of sandstone; at Elemore twelve and a half fathoms of limestone and ten fathoms and a half of sandstone; and at Eppleton only nine and a half fathoms of limestone and twenty-one fathoms of soft sandstone. The Elemore Pits are one mile south-west of the Hetton Pits, and the Eppleton about one mile north-east of the Hetton Pits." . In this part of the district the dip of the coal beds is towards the east, at the rate of one yard in twenty-two; but along the coast of Northumberland, and as far south as Manor Walls End*, at the mouth of the Tyne, they rise rapidly to the east, as well as more gently towards the west. On the other hand the coal-measures south of the river Wear continue to dip regularly to the eastward. Even at Hetton the high main coal had not recovered the good quality it was formerly known to possess on the Tyne; but the low main, a very inferior coal, from being brittle and breaking into small fragments before reaching the London market, is at Hetton and other Wear water collieries a coal of very superior quality. In all these mines the *low main of the Tyne* is called the *Hutton seam.* See section of Byker St. Anthon's Colliery, Geological Transactions, first series, vol. iv. pp. 41, 42.

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* This is the soft yellow sandstone which intervenes between the magnesian limestone and the coal-measures, so ably described by Prof. Sedgwick in the Geological Transactions, in his papers on the subject read in 1826, 1827 and 1828. [Abstracts of these papers were given in the Phil. Mag. and Annals, N.S. vol. i. p. 66, and vol. iii. p. 301.—EDIT.]

[To be continued.]

XXXIX. *Remarks on one of* Mr. Talbot's *proposed Philosophical Experiments. By* C. Wheatstone, *Esq.*

To Sir David Brewster.

Dear Sir,

IN the last Number of the Philosophical Magazine is inserted an article, entitled "Proposed Philosophical Experiments, by H. F. Talbot, Esq. M.P. F.R.S." Upon a part of this communication, headed "On the Velocity of Electricity," I feel it necessary to make the following observations.

My experiments were not made solely with a view to determine the velocity of the electric spark *passing through air* as Mr. Talbot has inferred, but, from the first, were intended to extend to the passage of electricity through solid conductors. In fact, the very first experiment of the kind which I made, and which was shown to the members of the Royal Institution on the evening my investigations on this subject were first made public by my friend Dr. Faraday, was one by which I endeavoured to show the deviation from a vertical line of two sparks simultaneously visible at the opposite ends of a metallic conductor. Beyond this, I have for several months past communicated very generally to my scientific acquaintance the details of an experiment upon a larger scale, which I hope soon to have an opportunity of executing, and which will detect and measure the velocity of electricity in its passage through a metallic conductor, though the rapidity of its transmission may exceed that of light: this I have proposed to effect, by increasing, in certain proportions, 1st, the velocity of the revolving mirror; 2ndly, the length of the conducting wire; and 3rdly, the accuracy of observing the deviation of the sparks from a vertical line. If I succeed in this point, it is obvious that we shall possess a means of directly measuring the relative conducting powers of metals, and of ascertaining numerous particulars respecting ordinary electricity which we at present have no means of determining.

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* Almost all coals with us are now designated Walls End, whatever seams may produce them.

Intending in the ensuing session to submit to the Royal Society all the results I have obtained, in reference to *a new optical means of measuring rapid motions, minute intervals of time, and feeble intensities of light*, I have hitherto refrained from publishing any incomplete statement of them; but I regret that this delay should have occasioned my experiments to be so far misunderstood, that one of the earliest which suggested itself to me, and which I have always considered to be of primary importance in the series, should be proposed elsewhere, several months afterwards, as an experiment yet to be tried, and be represented also as having entirely escaped my attention. I remain, Dear Sir, yours, &c.

Conduit-street, Hanover-square, C. WHEATSTONE.
August 2, 1833.

XL. *Experimental Contributions towards the Theory of Thermo-electricity. By Mr.* JOHN PRIDEAUX, *Member of the Plymouth Institution.* *

THE discovery of Professor Seebeck, that a bar of antimony, or of bismuth, heated in contact with a copper or brass wire, would affect the magnetic needle, was soon extended by chemists in this country and on the Continent to all the other metals which are of ready access; and a table of the thermo-electric order of these metals was soon published by Professor Cumming, and found to differ from the voltaic order, and also from that of conduction, whether of heat or electricity.

Other experimenters presently discovered thermo-electric currents in single masses of metal, which have been traced out with curious results †.

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* Communicated by the Author.

† [Papers on the thermo-magnetism of homogeneous bodies, by Mr. Sturgeon, will be found in Phil. Mag. and Annals, N.S. vol. x. pp. 1, 116, &c.—EDIT.]

M. Becquerel is, I believe, the only one who has investigated the comparative force as well as the order of arrangement of metals when acting in pairs; and having ascertained these points by a course of delicate experiments, he has been led to the inference that thermo-electricity is allied to the radiation of caloric.

The deep interest attached to a subject in which the mutual reaction of heat, electricity and magnetism is made almost tangible within a very narrow compass, led me to institute the following inquiries, with the hope of their suggesting other methods of elucidation.

I. Is thermo-electricity different from that derived from other sources: i. e. a different principle, or a different combination of principles?

II. Is it produced at the *expense* of caloric?

III. Is the radiation of heat, or any property dependent upon it, the proximate cause of thermo-electricity?

IV. Is the proximate cause connected with the conduction of heat?

V. Will not a hot bar brought into contact with a cold one of the same metal set electricity in motion; and if so, will the currents so produced bear any peculiar relation to those produced by pairs of different metals?

I. *Is there any, and what, difference between Thermo-electricity and that derived from other sources?*

1. The most characteristic distinction between electricity and the other imponderable fluids (if such they be,) is, its [sensibly] instantaneous transmission through considerable length of solid conductors. The very low tension of thermo-electricity renders this as close a test as any to which it can be subjected. Fifty feet of iron wire (one of the worst metallic conductors,) was cut into two lengths, through which (turned at the ends, to ensure metallic contact,) two other mercury boxes were connected with those of the magnetest [*]. A thermo-electric pair, of antimony and bismuth, heated at the point of contact, had their feet dipped, first into the mercury boxes of the magnetest, which produced a deflection of 80°; and were then removed to the other pair of boxes at the end of the wires, by which the deviation was reduced to 15°; and so repeatedly; the interposition of such a length of iron wire, between the excited metals and the magnetest, withholding $\frac{4}{5}$ths of the deviation (about $\frac{19}{20}$ths of the current): yet was the

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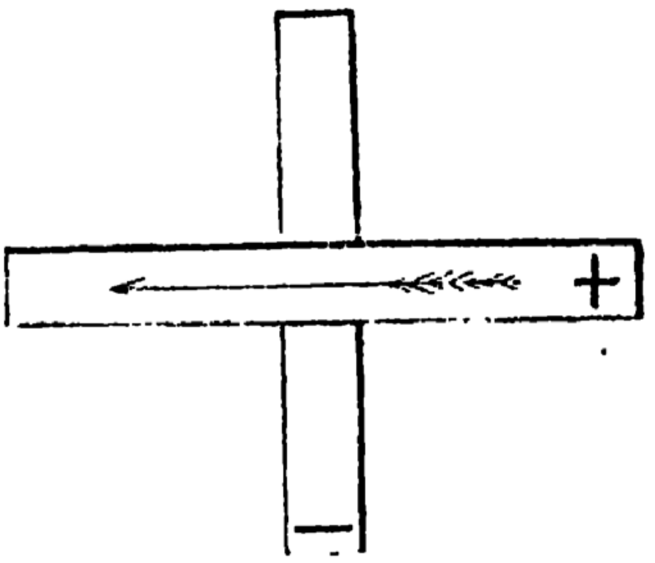
[*] I must be excused for resuming the use of this word: "galvanometer" would be hardly applicable in this instance.

instantaneous movement of the needle as evident in one case as in the other. So far, then, as promptitude of transmission through long wires is a distinction, thermo-electricity does not differ from the other kinds.

The magnetest employed in these experiments consists of a pair of sewing needles, 3 inches long, the lower one having just enough of predominance to give them terrestrial direction. The conductor, which passes only once between them, and returns beneath the lower needle, consists of four lengths of bright copper bell-wire laid together, not twisted, the ends amalgamated, and working into mercury boxes. A pair, of copper and tinned iron, by the heat of the fingers gives a deflection about 25°.

A guide-cross lies between the mercury boxes, to keep always before the eyes the relations between the current and the needle.

It is constructed of two slips of card, fastened in the centre, one over the other, with sealing-wax; one representing the magnet, the other the conducting wire. On the latter an arrow is drawn (on both sides) to indicate the direction of the current; on the former a line, also on both sides, to distinguish the marked pole.



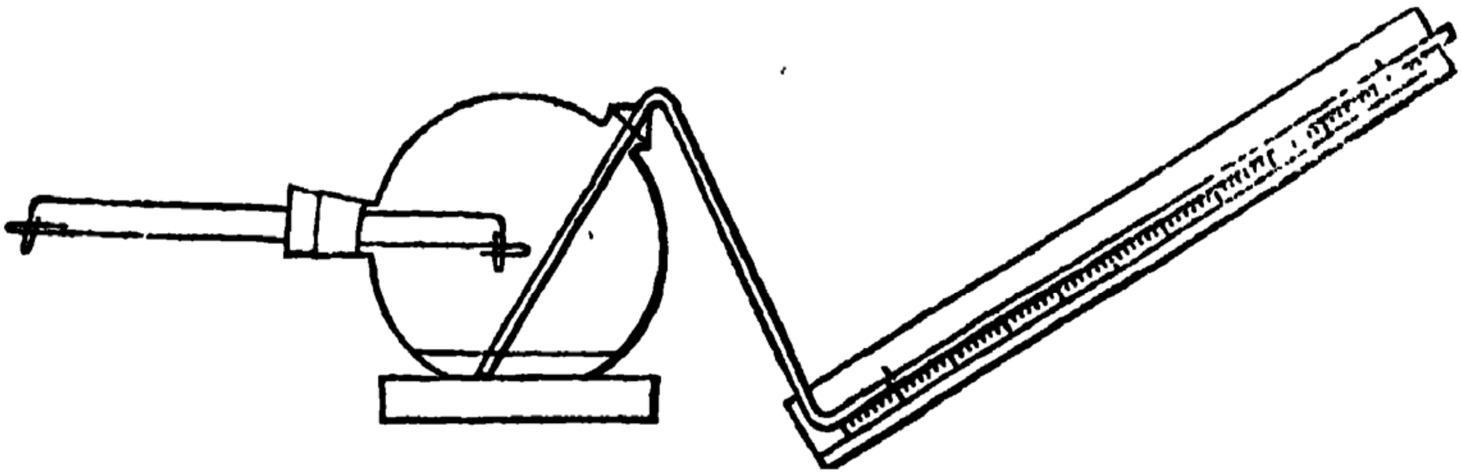
This cross turned over, on either of the slips, as an axis, or placed horizontally, vertically, or in any degree of obliquity, always exhibits the relation between the current and the needle: and being laid down, with its magnet corresponding with the position of the needle, it shows in what direction the current is passing over it; or turning it over upon the magnet as an axis, the reverse current is seen returning under it.

2. If there be any modification of electricity answering to that of Dr. Hare, a compound of electricity and caloric, this, from the mode of its generation, should be the one. But the same pair, of antimony and bismuth (1.), heated until they would drive the needle nearly round the circle, by the first impression, had not any appreciable effect on Mr. Harris's thermotest, though tried upon wires of various dimensions and different conducting powers. Thermo-electricity, then, seems to be no way distinguished as containing caloric.

Attractions and repulsions, or chemical decompositions would be precluded by its low tension; and these are its only apparent distinctions from the voltaic current, or from the electricity of the machine.

II. *Is Thermo-electricity produced at the expense of Caloric?*

3. Into the smaller neck of a tubulated receiver was fixed an open glass tube, reaching obliquely to the bottom, and bent, siphonwise, on the outside, so as to descend a little lower than the inner end: it was then again bent upward, nearly at a right angle, and to this rising end a scale was attached.



Into the receiver was poured water tinged with cochineal, and covered with a stratum of oil, to prevent evaporation by the heat. Suction being now applied to the end, the liquid remained, on the principle of the siphon, a few degrees up on the scale. A cork thoroughly soaked in grease being fitted to the open neck, the whole constituted a very sensitive air thermometer.

The cork was now perforated by two wires; one of copper, the other of tinned iron; the latter bent off at each end, so that it could be brought into contact with, or separated from the copper by a slight turn with the fingers. Both ends of each wire were doubled, flattened and polished to ensure good contact; they projected about three inches into the receiver, and nine inches outside.

Being now set in contact, the cork was drawn out, and the ends of the wires which entered the bulb were placed between a fold of sheet copper, and heated by holding them for a given time (25^s) over an Argand gas light, being turned over at 15^s, to equalize the heat in both wires. The sheet copper being now drawn off, the hot ends were thrust into the bulb, and the cork pressed in air-tight. The fluid rose quickly to about 90° (more or less, depending on slight differences in the heat, and in the quickness and force with which the cork was pressed in). After a few seconds, the liquid began again to descend, by the cooling of the wires. They were then separated, when the descent of the fluid was suddenly checked: on renewing the contact, it fell with renewed impetus; and so stopped and resumed its fall at every separation and renewal of the contact.

Other wires of the same and different metals were tried, and with many different corks; but the effect was always consistent, and indicated that the thermo-electric current did consume caloric in its production.

4. If this inference were true, it should follow that the wires heated to the same degree, should cool much more rapidly when kept in contact the whole time of cooling, than when kept all the time separate. They were therefore heated as before, and being thrust in, the fluid was allowed after rising to recede to 70°; from which its time of descent to 20°, i. e. 50°[*], was observed by a seconds watch,—the wires remaining in contact.

The same proceeding was repeated, the wires remaining separate.

Precision, for reasons above given (3.), was unattainable; and the conclusion was only to be drawn from the average of many experiments. This average gave 60^s for the closed wires, and *less* than 60^s for the separated ones; thus the cooling seemed retarded, rather than accelerated, by the contact, and consequent electrical current. All that repetition and examination could do to avoid discrepancy was attended to, and yet these experiments seemed to contradict the preceding ones.

5. The mode of interrogation was reversed. If caloric disappeared in the production of thermo-electricity, it was not unlikely that where caloric became latent, thermo-electricity might appear.

A glass siphon being suspended by the ends over a spirit lamp, tin was dropped in and melted there, until the siphon was nearly full; the ends were then connected with the poles of the magnetest by long and similar copper wires, tinned at the points. Although copper is (generally) positive to tin, no current ensued, because both ends were alike in contact with copper. A hot iron wire being now plunged into the tin, at one end of the siphon, gave a deflection of 6°; and when transferred to the other end, a deviation to the same amount, in the contrary direction; and so repeatedly (37.): thus manifesting a current when electricity was developed. A slip of tin was now plunged into one end; of course no deviation could be expected (37.). This tin was allowed to melt there, by which caloric must have become latent. The fusion was gradual, and in full contact with metal on all sides; the electricity, if any were produced, must therefore have been continuous, and could not have acquired tension. It would also have taken the copper, in preference to the tin, and the needle would have deviated accordingly.

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[*] Not degrees of Fahrenheit, or of any other standard scale; for the heat of the air about the wires must have been very different from that about the glass.

But no deviation occurred.

Mercury, which is pretty near tin in thermo-electric power, was then substituted; and being heated to the melting point of tin, a warm iron wire was plunged in, which gave a deviation of 5°: but iron not coming well into contact with mercury, a tinned iron wire was tried, by which a deviation of 6° was obtained as before (5.). In this slips of tin were successively melted; but without any manifestation whatever on the needle.

Here we have no evidence of thermo-electricity, on the disappearance of caloric; in the preceding experiments we had no evidence of the disappearance or abstraction of caloric, in the production of thermo-electricity. To what, then, are we to attribute the contrary evidence of the previous experiments, as the apparatus was certainly air-tight?

6. When the wires were separated, the whole heated surface was exposed to the air in the bulb; and the portion between the heated ends would be most expanded, and least affected by the cooling influence of the glass. When they were in contact, this, the most effective part of the heated surface, was covered up. Hence a momentary expansion upon opening, and contraction upon closing the contact of the wires.

But since when thus in contact they cool less quickly, in proportion as they have less effect upon the air; so the whole time of descent, for 50° of the scale, should be greater, rather than less, when closed than when open.

If this were the true explanation, homogeneous wires should answer as well as those of different metals. Accordingly, two copper wires were passed through a cork, bent and heated as before (3.); and the descent of the fluid was found to be suddenly stopped and resumed, at the instants of opening and closing the contact, just as when the wires were of two different metals.

7. The evidence is therefore opposed to the expenditure of caloric in the production of thermo-electricity, and to the converse. Yet I thought the opposing experiments worth stating, (as they were not unlikely to occur to others,) to show that they had been tried, and shown to be inconclusive.

III. *How far is the Radiation of Heat, or any collateral property, concerned in the Development of Thermo-electricity?*

8. The result of M. Becquerel's exact researches, above quoted from the *Ann. de Chim. et de Phys.*, Aug. 1829, is, that the only known property of heat in which the metals take

the same order as in thermo-electricity, is that of radiation; and therefore he supposes that two pieces of hot metal, in contact, radiate in the same proportions as they would if separate and exposed; and that thermo-electricity is a result of this inter-radiation.

This is certainly applying to the term radiation a new meaning, very little analogous to its ordinary acceptation*. Yet the same quality, whatever it may be, which occasions radiation to be more or less copious, may also determine a superficial communication, proportionately copious, though different in mode. We have evidence, too, that the superficial absorbent power for heat, is proportionate to the radiating power; and it seemed not very improbable that here might be found some trace towards the solution of the problem.

9. If the effects were due to any cause connected with, or related to radiation, they should be modified by alterations in the surfaces; and by making the surfaces of a thermo-electric pair identical, their reaction should be greatly weakened, if not neutralized altogether.

An iron and a copper wire, of the same size (10 × $\frac{1}{16}$ inch), were well tinned from end to end, and twisted together for an inch. A similar pair, untinned, but polished, were similarly joined; and the feet tinned for ½ an inch, that the contacts with the mercury might be alike. Instead of the tinned wires having their efficacy impaired, they gave greater deflections than the clear ones. This unexpected fact, verified in a great number of ways, led to a long course of experiments; the general bearings of which will occupy a subsequent section (VIII.).

I am at a loss to reconcile with this fact any cause connected with radiation.

10. To try radiation more directly, and on a larger scale, a plate of tinned iron was wired round with tinned iron wire, which projected at one corner, for a conductor. A sheet of soft tin was also wired round with copper wire, projecting in the same manner, and for the same purpose. The sheet tin being laid on the tin plate, with a leaf of bank post paper between, to prevent contact, the conductors were plunged into the mercury of the magnetest, and a couple of lamps placed under the tin plate. The whole soon became hot, without deflecting the needle. The tin sheet was now lifted off, the paper removed, and a few threads spread about the

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* [It appears to us, on the contrary, that M. Becquerel here uses the term radiation in a sense perfectly similar to its ordinary acceptation: see Sir J. Herschel's remarks on the process by which radiation and conduction slide into each other, in his Prel. Disc. on Nat. Phil.—EDIT.]

tin plate, to keep the sheet tin from touching it, whilst their surfaces should be fully exposed for radiation, at an extremely small distance. No deflection ensued on thus replacing the sheet tin; but on putting a slip of tin-foil in contact with both, the needle started off 25°.

A plate of polished copper was similarly used, first with sheet tin, afterwards with the tin plate; but with no effect whatever, in either case, until metallic contact was made.

The total inefficacy of such large radiating surfaces, added to the result of the preceding experiments, discourages the hope of tracing the cause of thermo-electricity to any superficial action analogous to radiation; and more convincing evidence to the same effect will appear (20. *et seq.*).

In fact I have been unable to find any table of radiation corresponding with Becquerel's thermo-electric order.

IV. *Is the proximate cause of Thermo-electricity in any way connected with Conduction?*

12. Having previously found reason, from the experiments of others as well as my own, to conclude that the heat produced by electricity in metallic conductors (and, with due allowance, in liquids and air,) is in the order of, and probably in proportion to the resistance it encounters in the body heated, whether that resistance was owing to inferior conducting property or diminished thickness, I was led by the following experiment, compared with some of Becquerel's, to suspect a reciprocity of action, such that electricity, restricted in its passage, producing heat; so heat, allowed to flow more freely, might produce electricity.

The circuit from a voltaic coil was divided into 5 parts, the two ends next the coil, and the 5th or middle piece, being of stout copper wire, the ends each 2 feet, the middle piece 4 feet long; the connexions from each end to the middle piece were fine silver wire $\frac{1}{100}$th of an inch thick. Thus no electricity, positive or negative, could reach the middle piece except through these fine wires.

The charge being now made such as to warm the silver wires, a magnetic needle was moved along the circuit, which was laid (ap ortion of the middle piece, of course, excepted,) in the magnetic meridian. Its equidistance from the conducting wire was ensured, by the card, on which it was supported, being kept always in contact therewith. When over the copper, either of the middle or end pieces, the divergence was 36°; when over either of the silver wires, 26°. Thus heat seemed to be produced, by restriction of the current, on entering the silver wire; and the full electrical effect appeared to be restored when the obstruction was overpassed.

13. Becquerel found (*ubi supra*) that a circle of platinum wire, of equal diameter throughout, produced no current when heated in any part; but a knob being made in the circle, and heat applied near it, on either side, a current set always from the heated point towards the knob, where the heat had room to diffuse itself.

Also, that on soldering together the ends of his magnetest wire, no current ensued, when the point of junction was heated even to redness; but on touching the wire on either side of the heated point with a cold piece of the same metal, the current immediately set towards the part so touched.

14. It is true that the tables of conduction for heat and electricity, contrasted by Cumming with that of thermo-electricity, deny the presumption of any connexion between them. But there would appear to be two properties of conduction for caloric,—promptitude and final efficacy. For although we handle platinum wire, at two inches distance from a red-hot point, in operations with the blowpipe, which hardly any other metal will admit of, yet M. Despretz found that a rod of platinum *eventually* became hotter, at a given distance from a given source of heat, than any other metallic rod of equal dimensions; and if thermo-electricity be influenced by conduction at all, it must be by promptitude of conduction, for very short distances: whilst the tables hitherto published refer to a property compounded of the two, or intermediate between them.

A copper and an iron wire, drawn so that equal lengths corresponded to the atomic weights, were bound with thread for an inch each, cut off at ½ an inch above the binding,—this ½-inch bent over in the form of a hook, and the end filed flat. The two wires, thus prepared exactly alike, were fixed to a little wire frame, to keep them parallel and separate, and the filed ends dipped into melted lard. A couple of orange peas, of equal weight and size, were also dipped in the lard. When cold, the cohesion of the lard was sufficient to support the little balls against the ends of the wires. The bound part of the wires was now plunged into warm mercury, and the ball soon fell from the *copper* by fusion of the lard. The wires were then cooled by plunging the bound part into cold mercury, and the balls interchanged. On replunging into warm mercury, the ball fell again from the copper; and this always took place on repeated interchange of the balls, and alternation of cold and heat; the ball falling every time from the copper, until the mercury was cooled so far that the lard would not melt, and so that neither ball would fall. This was decisive in favour of the conducting power of copper, down to a very low heat, and at ½ an inch distance.

15. To try at still shorter distances, two other lengths of the same wire were twisted together for ½ an inch, then separated for an inch, and the other ends brought near together, for holding in a pair of cold pliers. (The twist was tarnished, to prevent metallic contact between the mercury and copper, which also was the object of the thread-binding in the former part of the experiment.) Spots of lard were now placed, at equal distances from the twist, on the iron and on the copper, and the twist plunged into the warm mercury. The lard became transparent (by melting) first on the copper: and this on repetition, whatever the distances, down to ⅛th of an inch; and whatever the temperature, so long as it was sufficient to melt the lard at all. Varying the sizes and proportions of the wires, tinning their surfaces, to prevent unequal waste of heat, *in transitu*, by radiation, did not alter the result. In every instance, for equal distances, the lard first became transparent upon the copper wire; which is thereby proved, for low heats and short distances, to retain its character as the better conductor for caloric.

16. To ascertain the conducting powers for electricity of very low tension, a pair of iron and a pair of copper wires, atomically proportioned as before, and each 2 inches long, had ½ an inch at each end bent down at right angles, and the points tinned, for equal contact with the mercury. Through these wires, two other mercury boxes were connected with those of the magnetest, an iron and a copper wire being employed on each side. Into the further boxes dipped the ends of a thermo-electric pair, of copper and tinned iron, of which the other ends were twisted together, and the twist was exposed to an equable heat, until the needle became stationary at 18°.

The short iron wires were now removed, leaving the conduction to be carried on by the short copper ones only: no appreciable change took place in the deflection.

The iron wires being now replaced, the copper ones were removed, leaving the conduction entirely to the iron. The needle receded more than a degree, and sometimes nearly 2°, confirming the superiority of copper as an electrical conductor, for extreme low tension and very short distances.

17. But as heat also enters into the question, the apparatus remaining as described, a spirit-lamp was brought under the middle of the copper leg of the thermo-electric pair, and continued there until the point acted upon by the flame became red hot. Little or no change took place in the deflection.

The lamp being then removed to the iron leg, the needle receded 2° before the heat had attained redness. Thus the conducting power of iron suffered more from heat than that of copper.

18. Iron being thus proved to be inferior in conducting power, both for heat and electricity, in circumstances directly in point, to copper, cannot owe its thermo-electric superiority to that property; and, extending the analogy to other metals, the inference at the beginning of this section would seem to be refuted. But the following section will bring the question before us in a different point of view.

[To be continued.]

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 18. A PAPER was read, entitled, "On Improvements in the Instruments and Methods employed in determining the Direction and Intensity of Terrestrial Magnetism." By Samuel Hunter Christie, Esq. M.A. F.R.S.

The tedious nature of the observations by which the direction and intensity of the terrestrial magnetic force are determined, and the uncertainty attending the results when obtained, have long been a subject of regret to all who are engaged in the investigation of the phænomena of terrestrial magnetism. Sensible of this, the author's attention has at different times been turned to the improvement of the instruments employed for these purposes; and in this communication he proposes methods by which he considers that these instruments might be so improved that the results should be obtained with greater facility and also with greater certainty. The uncertainty attending the results obtained with the dipping needle, as at present constructed, arises principally from the two sources, friction upon the axis, and the want of coincidence of the needle's centre of gravity with the axis of motion; the latter rendering necessary the inversion of its poles. The author suggests a method by which he considers that, probably, the friction may be diminished; but he has principally directed his attention to obviate the necessity of the inversion of the poles.

In order to remove the practical difficulty attending the adjustment of the centre of gravity to the axis of motion, an operation in which the artist rarely, if ever, completely succeeds, the author proposes to dispense with this condition; and shows how the dip may then be determined, without the necessity of inverting the poles of the needle, the position of its centre of gravity having been determined previously to its being magnetized. The advantages attending the method proposed by the author are not, however, restricted to the determination of the dip with greater accuracy and greater facility: a further and still greater advantage attending the use of a dipping needle on the principle he proposes, is, that a measure of the terrestrial magnetic intensity will be obtained by the same observations, which give the dip; so that, by this means, the observations usually required for that purpose, and which are of the most tedious nature, will be avoided. To effect both these objects in the most convenient manner, he proposes that the needle should be so constructed that its centre of gravity should be out of the axis of motion, in a line perpendicular to that axis and to the axis of the needle. The requisite formulæ for determining the dip and the measure of the terrestrial intensity, in this case and also when the centre of gravity is in any other position, are investigated in the paper. Mayer had previously pointed out that the dip might be determined by means of a needle having its centre of gravity out of the axis of motion, and had given the formulæ requisite for that purpose. His object, however, does not appear to have been the same as our author's,—the avoiding in all cases that source of inaccuracy, the inversion of the poles of the needle,—but simply the determination of the dip, whether the centre of gravity of the needle were made to coincide with the centre of motion, or not: the determination of a measure of the terrestrial intensity, by such means, does not appear to have entered into his contemplation.

As another form in which the same principles might be advantageously applied, the author proposes that two needles, similar in all respects, should be placed on the same axis; and points out how, by means of such a compound needle, both the dip and intensity might be determined by independent methods, so that the agreement of the results would afford a test of the accuracy of the adjustments and of the observations. He considers that the knife-edge support, which has recently been adapted to a dipping needle, would be peculiarly applicable to a needle of this construction. The sensibility of such a needle would be much greater than that of any hitherto constructed, and the utmost delicacy would be required in the adjustments; but if the needle were accurately constructed, and due care were taken in the magnetizing, and in making the adjustments and observations, the author expects that the dip and intensity would be determined to a degree of certainty hitherto unattained.

The advantages proposed to be derived from the use of a dipping needle on the principle described in this paper, are, that as the dip would be obtained without inversion of the poles, the results would be less liable to error than when that operation is necessary, and the observations would be made in less than half the time usually required; and that a measure of the intensity of terrestrial magnetism would be obtained from the same observations which give the dip, the intensity of the force being thus always determined by means of the same needle, and at the same instant as its direction.

May 2.—A paper was read, entitled, "Essay towards a first approximation to a Map of Cotidal Lines." By the Rev. William Whewell, M.A. F.R.S. Fellow of Trinity College Cambridge.

The general explanation of the phænomena of the tides originally given by Newton, although assented to by all subsequent philosophers, has never been pursued in all the details of which its results are susceptible, so as to show its bearing on the more special and local phænomena, to connect the actual tides of all the different parts of the world, and to account for their varieties and seeming anomalies. The first scientific attempt that was made to compare the developed theory with any extensive range of observations, was that of Daniel Bernouilli in 1740: the subject has since been pursued by Laplace and Bouvard, and still more recently by Mr. Lubbock. But the comparison of contemporaneous tides has hitherto been unaccountably neglected: and to this particular branch of the subject the researches of the author are in this paper especially directed; the principal object of his inquiry being to ascertain the position of what may be called *cotidal lines*, that is, lines drawn through all the adjacent parts of the ocean where it is high water at the same time; as, for instance, at a particular hour on a given day. These lines may be considered as representing the summit or ridge of the tide-wave existing at that time, and which advances progressively along the sea, bringing high water to every place where it passes. Hence the cotidal lines for successive hours represent the successive positions of the summit of the tide-wave, which in the open sea travels round the earth once in twenty-four hours, accompanied by another at twelve hours' distance from it, and both sending branches into the narrower seas. Thus a map of cotidal lines may be constructed, at once exhibiting to the eye the manner and the velocity of all these motions.

Although the observations on the periods of the tides at different places on the coast and different parts of the ocean, which have been at various times recorded, are exceedingly numerous, yet they are unfortunately for the most part too deficient in point of accuracy, or possess too little uniformity of connexion to afford very satisfactory results, or to admit of any extended comparison with theory. With a view to arrive at more correct conclusions, the author begins his inquiry by endeavouring to determine what may be expected to be the forms of the cotidal lines, as deduced from the laws which regulate the motions of water: and he proceeds afterwards to examine what are their real forms, as shown by the comparison of all the tide observations which we at present possess.

The paper is divided into five sections. In the first the author treats of cotidal lines as deduced theoretically from the known laws of the motion of fluids. On the supposition that the whole surface of the globe is covered with water, the cotidal lines would coincide with the meridians, and would revolve round the earth from east to west in something more than twenty-four hours, with a velocity of nearly 1000 miles an hour at the equator. The form and the regularity of these lines would be materially affected by the interposition of land in different parts of this ocean, whether in detached islands, or groups of islands, or large continents, occupying a considerable portion of the surface. In these cases the primary wave will be broken, deflected and variously modified, so as to give rise to secondary or derivative tides, sometimes separating into branches, and producing points of divergence; sometimes uniting at various places, or points of convergence; and at other times producing, by more complex combinations, various phænomena of interference, and other apparently anomalous results. Such is the general character of the tide-waves that actually proceed along the coasts of the Atlantic: and the modifications in their course and velocity are still more perceptible in bays, gulfs, and narrower channels and inlets of the sea, as well as in their progress along rivers. The author traces in detail the effects which these different circumstances may be expected to produce. He adverts to an important distinction which has frequently been lost sight of, between the progressive motion of the tide-wave and the actual horizontal motion of the water, or tide-current; motions which do not bear any constant relation to one another. Hence the change in the direction of the current does not invariably indicate the rise or fall of the water.

In the second section he examines the causes which have led to inaccuracy in making observations on the tides; the first of which is dependent on the circumstance just mentioned, of the occasional want of correspondence between the times of *high* and of *slack* water; the former referring to the moment of greatest elevation, the latter to that when the direction of the current changes. The other causes of error are derived from the change which takes place in the course of the day in the moon's angular distance from the sun; from the half-monthly inequality in the establishment, arising from the relative position of the sun and moon during each lunation; and from the necessity that exists of making a correction for what may be termed the *age* of the tide; that is, the interval of time which has elapsed between the period of the origin of the wave and the time of its actual arrival at the place of observation.

The third section, which forms the chief bulk of the paper, is occupied by a statement and discussion of the tide observations now extant, and which the author has, with great industry, collected from a variety of sources, both of published accounts, and of manuscript documents preserved in the Admiralty. Commencing with the tide-waves, first of the eastern and then of the western coasts of the Atlantic, he follows them to the Northern sea, and to the different coasts of the British islands, and of the German Ocean. He passes next to the examination of those of the Southern Atlantic at Cape Horn and the adjacent coasts; thence tracing them, as far as the present imperfect data will allow, along the western shores of the American continent, to the central parts of the Pacific, and in their progress across the Australian and Indian Oceans. He likewise examines the condition of the tides in rivers, as to the magnitude and velocity of the undulations, the occasional production of a high and abrupt wave, or *bore*, and as to the influence of the natural stream of the river upon the different periods of elevation or depression of the water.

The fourth section contains general remarks on the course of the tides, suggested by the preceding review of the phænomena they present; on the velocity of the tide-wave; on the form of the cotidal lines; on the currents which attend the tides; on the production of revolving currents; on the magnitude of tides; and on the constancy of the cotidal lines. He adverts also to some peculiarities resulting from interference, such as the differences of the two diurnal tides, and occasionally the occurrence of single day tides.

In the concluding section the author offers various suggestions respecting the most eligible mode of making observations on the tides, and of correctly reducing them when made.

GEOLOGICAL SOCIETY.

February 27.—A paper, commenced during the last Session, entitled "Description of Parts of the Kingdoms of Valencia, Murcia, and Granada, in the South of Spain," by Capt. Cook, R.N. F.G.S., was concluded.

A paper entitled "Observations relative to the Structure and Origin of the Diamond," by Sir David Brewster, K.C.H. LL.D. F.G.S. &c., was then read.

In the year 1820, the author communicated to the Royal Society of Edinburgh a singular fact relative to the structure of the diamond, accompanied with some conjectures respecting the origin of this remarkable gem:—the present essay may be viewed as a continuation and extension of the same inquiry.

The author refers to the remark of Newton, that amber and the diamond have a refractive power three times greater in respect of their densities than several other substances; and he quotes Newton's conjecture, founded on that remark, of the diamond being probably, like amber, an unctuous substance coagulated. In proof of the intimate relation between the inflammability and absolute refractive power of bodies, Sir D. Brewster adds the facts, that sulphur and phosphorus exceed even the diamond in absolute power of refraction, and that these three inflammables stand before all solid and fluid substances in their absolute action upon light.

Another close analogy between the diamond and amber, independently of their like locality and carbonaceous nature, was traced by the author in their polarizing structure. Both of these minerals contain within their substance small cells or cavities, filled with air, the expansive force of which has communicated a polarizing structure to the parts in immediate contact with the air. The description of this structure, which is displayed from sectors of polarized light encircling the globule of air, is illustrated by drawings.

The author contends that the peculiar polarizing power around the cavities in amber and in the diamond must have been occasioned by the expansive force of the confined substance, supposed to be gaseous, compressing the sides of the cells, while the substance of the minerals was in a soft and yielding condition. A similar structure may be produced in glass, or in gelatinous masses, by a compressing force, propagated circularly from a point.

Having thus shown that the diamond was at one time in a soft or pasty state, the author argues that this state was not produced by igneous fusion. For in his laborious examination of the cavities in crystals, both natural and artificial, such as topaz, quartz, amethyst, chrysoberyl, &c., and in salts, he observed the condition of many thousand cavities; but in no case, neither in crystals formed by means of igneous fusion nor by aqueous solution, did he observe a single cavity in which the expansible fluid within had communicated a polarizing structure, similar to that around the cavities in the diamond. He believes, therefore, that the softness must have been that of semi-indurated gum; and that the diamond was derived from the decomposition of vegetable matter, as is admitted to have been the case with amber. The crystallized condition of the diamond is not to be considered as decisive against this inference, since the mineral called mellite has a distinct crystallized form, while its composition and locality attest a vegetable origin*.

A notice "On the Occurrence of the Bones of Animals in a Coal-mine in Styria," by Professor Anker, of Joanneum in Gratz, was then read.

March 13.—A paper, entitled "Geology of the Environs of Bonn," by Leonard Horner, Esq. F.G.S. F.R.S. &c., was read.

The district described by the author lies on both sides of the Rhine,—the Siebengebirge, or Seven Mountains, constitute the chief feature; and the highest point in the group, the Oelberg, is 1369 English feet above the level of the sea. It possesses peculiar interest to the geologists of England, as being the nearest point where volcanic phænomena, approaching in character to those of modern times, can be seen.

The lowest stratified rock is grauwacke, which seems to belong to the later ages of that deposit, and to approach in character, in some parts of the district, to the old red sandstone. There are no associated beds of limestone. The strata are in general highly inclined, but they are found at all angles; and there is neither uniformity in the strike nor in the dip; the strike is most usually N.E. and S.W., the dip more frequently S. than N. In the immediate neighbourhood of the Siebengebirge, the strata are thrown up in all directions, evidently by the eruption of the volcanic matter.

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* [As the subject of the origin of the diamond was discussed in our pages some years ago, we annex an historical remark or two to the above abstract of Sir D. Brewster's paper. In the communication to the Royal Society of Edinburgh, alluded to in the abstract, it was argued that the original compressible state of the diamond could not have arisen from the action of heat, on account of "the nature and recent formation of the soil in which it is found." Mr. Brayley, when discussing the arguments in favour, respectively, of the vegetable and the mineral origin of this gem (in a notice headed "*Origin of the Diamond*," published in the Phil. Mag. and Annals, N.S., vol. i. p. 147—149, and signed E. W. B.), observed, that the discovery, subsequently to Sir D. Brewster's researches, of a diamond in a matrix of brown ironstone occurring in beds subordinate to chlorite slate in the Brazils, and the information, also subsequently promulgated, that the diamonds of Southern India are found in an ancient breccia, removed Sir David's objection on this point. Mr. Brayley inferred, therefore, since, for aught that had been advanced to the contrary, the compressible state once possessed by the diamond might have resulted from the action of heat, that the optical characters discovered by Sir D. Brewster, did not necessarily refer its origin to the vegetable kingdom.

In the paper read before the Geological Society, however, Sir D. Brewster shows, in a direct manner, by comparing the optical characters of the diamond with those of other crystallized substances, that the original soft or pasty state of the former could not have been produced either by igneous fusion or by aqueous solution. The evidence now adduced on this point is therefore quite independent of the geological situation of the gem; and it would appear to be impossible to refer it to any other than a vegetable origin, as originally suggested by the author of the paper.

We observe that Dr. Thomson's statement of the geological situation of the diamond (Inorg. Chem. vol. i. p. 156.) is derived from the above-cited notice in the Phil. Mag. and Annals.—Edit.]

The whole of the later secondary series is wanting, and the grauwacke strata are covered, unconformably, by a tertiary deposit, consisting of beds of sand, sandstone, clays, and lignites, which collectively constitute a brown coal formation. This is covered by an extensive bed of gravel, and above the gravel is a loosely coherent, sandy loam, containing land shells of existing species, and called in the Rhine valley, *Loess.* From under the grauwacke there have burst forth a variety of unstratified rocks, consisting of various modifications of trachyte, trachytic tuff, basalt, and other modifications of trap. The main body of the Siebengebirge is composed of these volcanic rocks.

There are many varieties of the trachyte, from a highly crystalline rock, with separate crystals of felspar of great size, very like a large-grained granite, to a compact stone of uniform structure, like compact felspar or phonolite. The trachytic tuff also assumes various appearances, from that of a coarse conglomerate to a white earthy substance, scarcely distinguishable from chalk. There is no evidence of the trachyte having flowed in a stream, and the author saw it only in one place in the form of a dyke. There are several varieties of trap, but the most common is a compact black basalt, in many places in perfect columns. There are numerous dykes of basalt. A remarkable eruption of trap tuff, penetrated by basaltic dykes, occurs at Siegburg, where three cones, of about 200 feet in height, rise abruptly in the midst of an alluvial plain, nearly on a level with the Rhine.

The author points out the affinity which Von Buch has shown to exist between the mineral composition of all the unstratified rocks, and how a series of insensible gradations could be formed, through trachyte and the trap family, from granite to modern lava. He shows that a suite of specimens could be collected in the Siebengebirge, passing insensibly from large-grained white trachyte to compact black basalt; and that these hills afford many interesting facts corroborative of the opinion advanced by M. Gustave Rose respecting the identity of hornblende and augite. Notwithstanding, however, this connexion between the several volcanic rocks, the author points out distinct evidence of different epochs of formation among them. He is of opinion that the greater part of the trachytic tuff was the first ejected; that it was similar to those showers of scoriæ and ashes which frequently precede the eruptions of streams of lava; and that it is not, as some previous writers have supposed, a rock recomposed from the disintegration of pre-existing trachyte. He saw the trachytic tuff traversed in one place by a dyke of trachyte, and it contains numerous balls, like volcanic bombs, of varieties of trachyte, quite distinct in character from any found *en masse*. It is traversed in many places by trap dykes; and as these last are also found traversing solid trachyte, the subsequent eruption of the trap is demonstrated. He discovered no instance of the recurrence of trachyte after basalt had begun to flow. There is on the side of the Rhine, opposite to the Siebengebirge, an extinct volcano, of comparatively modern date, the Rodderberg, composed of cinders and scorified rocks. The crater is about a quarter of a mile in diameter, and a hundred feet deep: a farm-house, surrounded by corn-fields, stands in the middle of it.

The brown coal formation is composed of beds of loose sand, sandstone, and compact siliceous conglomerate, which often, in mineral structure, cannot be distinguished from many varieties of grauwacke; of clay, abounding with balls and layers of clay ironstone; and of many varieties of lignite, from the state of a light brown earth, to a black compact shining mass, or jet. All of these are frequently met with in thick beds, and the lignite is most extensively worked for fuel. They contain numerous impressions of leaves, and stems of trees are very abundant. With the exception of casts of *Lymnæa* and *Planorbis*, in an opake white chert, of very limited extent, no shells, fresh-water or marine, nor any remains of mammalia or birds, have been found in any part of the formation; but in some beds of the lignite, impressions of fresh-water fishes, the *Leuciscus papyraceus* of Agassiz, are found in great abundance, and there have also been found extinct species of frog, salamander, and triton, together with remains of insects, which Professor Goldfuss considers to belong to the genera *Lucanus*, *Cerambyx*, *Anthrax*, *Cantharis*, and eight others.

The author submitted the specimens which contain impressions of leaves, to the examination of Professor Lindley. Most of them are in too imperfect a state to admit of any accurate determination, but they consist generally of casts of portions of dicotyledonous leaves; and among them are two species, the existence of which is sufficient to determine the relative age of the formation, and, with great probability, the then warm climate of the North of Germany, viz. *Cinnamomum dulce* and *Podocarpus macrophyllus*: there are besides impressions of leaves very clearly belonging to the Palm. It is remarkable, however, that a recent examination by Professor Nöggerath of Bonn, and M. Cotta of Heidelberg, of an extensive suite of the *woods* found in this brown coal formation, did not disclose a single instance of a monocotyledonous tree.

A vast deposit of gravel, chiefly composed of quartz, but containing also a few fragments of basalt, trachyte, transition limestone, and *bunter sandstein*, lies over the brown coal formation, sometimes being only a thin covering, at others attaining a thickness of 125 feet. It is very distinct in character from the gravel now forming the bed of the Rhine, and is older than some of the volcanic eruptions, for a patch of it rests on the edge of the crater of the Rodderberg, covered by volcanic ashes.

The author next proceeds to point out what he considers the relative age of this brown coal formation, a task extremely difficult, from the almost total absence of shells, and the imperfect state of the means of determining an epoch of formation by fossil plants. By previous writers it has been assigned to the plastic clay of the Paris basin; but it appears to the author to possess no other character of identity than the mineral composition of some of the beds, and the occurrence of lignite, which prove nothing as to age. The amphibious animal remains resemble those of the great fresh-water deposit of Œningen; but the few shells which occur, and the plants, are identical in species with many of those occurring in the older fresh-water beds of Alx in Provence. It seems to be very clear, that it is an exclusively lacustrine deposit, and the organic remains, the only evidence of age to be relied on where there is none from superposition of other beds, imperfect as they are, would seem to indicate a more modern date than the plastic clay. The author states that a distinguished geologist of Bonn has expressed his belief that it is even older than the chalk; but that although the opinions of that experienced observer are entitled to great respect, he cannot reconcile the phænomena described with anything known respecting the secondary rocks.

The determination of the age of this brown coal formation is of the highest importance, as fixing the periods of eruption of the extinct volcanos of the Lower Rhine; for the author of this paper shows, that the trachytic tuff contains leaves of plants identical with those found in the clay and sandstone deposits; that extensive layers of trachytic tuff are interstratified with the beds of the formation in many places; and that in one situation a mass of basalt, thirty feet thick, lies upon beds of coal thirteen feet in thickness. The conclusions which the facts appear to the author to justify, are, that there existed a vast fresh-water lake, in which the brown coal beds were deposited; that *during* that deposition volcanos burst forth at the bottom of the lake, as they do now at the bottom of the sea; and that a continuance of volcanic action, or of elevatory force, raised the Siebengebirge after the deposit had ceased,—perhaps at the very time when the basalt or trap eruptions took place, since near the summit of the Mandeberg, a columnar basaltic cone, there is a patch of brown coal beds at the height of nine hundred feet above the surface of the Rhine.

The last great formation, if it may be so termed, of this district, lying *upon* the gravel in which the present bed of the Rhine is cut, is that most remarkable deposit the *Loess,* a friable sandy loam, full of existing species of land shells, without river shells, and without plants, but containing bones of the *Elephas primigenius* and *Rhinoceros tichorinus.* It is found in detached masses, of vast thickness, but without any signs of stratification, and sometimes at a height of 600 feet above the Rhine, and may be traced with scarcely any interruption from Bonn to Basle, a distance of 250 miles. The author states that all the facts yet observed with respect to it, lead him to conjecture that its origin may have been owing to the sudden bursting of an extensive lake situated somewhere between Constance and Basle, and that subsequent denuding causes have carried away the enormous mass of matter deposited by this sea of mud, leaving only detached portions as monuments of the passing of the mighty torrent.

March 27.—The first part of a memoir "On the sedimentary deposits which occupy the western parts of Shropshire and Herefordshire, and are prolonged from N.E. to S.W., through Radnor, Brecknock, and Caermarthenshires, with descriptions of the accompanying rocks of intrusive or igneous characters," by Roderick Impey Murchison, Esq. F.G.S. F.R.S. &c., was read.

After adverting to the want of definite knowledge of the order of succession, and of the fossiliferous characters of those great deposits anterior to the old red sandstone, and commonly called transition rocks (fossiliferous grauwacke, De la Beche), the author states that the task of attempting to separate them into distinct formations, was first suggested to him by the very clear and perfect exhibition of their details in the country under review.

The present work has already occupied large portions of the two last summers, and has for its basis the maps of the Ordnance survey, coloured geologically by the author. To the Master General of the Ordnance, to Col. Colby, Capt. Robe, and the Officers of His Majesty's Map-office, for their assistance in supplying him with good geographical data, and also to Mr. Budgin, one of the Field-Surveyors of the Ordnance, the author acknowledges his obligations. He next adverts to the unpublished yet valuable observations of Mr. Arthur Aikin, made many years ago in the north-eastern portion of the country described; and he further expresses his sincere thanks to many resident gentlemen who assisted him in his observations.

The memoir is divided into three parts. The first contains an account of the overlying deposits of new red sandstone, coal measures, mountain limestone, and old red sandstone, each of which is in contact with, or contiguous to, the transition rocks (grauwacke series) in some portion of the region described. The second and most extensive part explains the subdivisions and relations of the grauwacke series as exhibited within a zone of country extending from the Wrekin near Shrewsbury on the N.E., to the mouth of the Towey, near Caermarthen, on the S.W.; and the third part is to be devoted chiefly to the consideration of the rocks of intrusive or igneous characters, and their effects upon the associated strata.

Part I. On the New Red Sandstone, Coal Measures, Mountain Limestone, and Old Red Sandstone.

1. *New red sandstone.*—This is the youngest secondary formation in contact with the transition rocks, and is exhibited on both sides of the Severn, near Shrewsbury, being superposed to coal measures, to various members of the grauwacke series, and to trap rocks of different characters, in all of which situations it is undisturbed.

The oldest strata of this formation are compared with the *rothe-todte-liegende* of Germany, or the older new red of the North of England, and are shown to underlie a dolomitic conglomerate at Alberbury and Cardeston. The superior members on the north bank of the Severn, consist of fine-grained sandstones, for the most part red, but offering at Grinshill, 7 miles N.E. of Shrewsbury, a subordinate, white building-stone, of excellent quality. Small quantities of the ores of copper and cobalt, are mentioned as occurring in the formation near Grinshill and Hawkstone, &c. Much sulphate of barytes and decomposed pyrites are diffused through the mass of these rocks. No organic remains have yet been discovered in them.

2. *Coal Measures.* *a. Coal field of Coalbrookdale.*—This coal-field is stated to rest at Steeraways and near Little Wenlock, on a thin band of limestone, which, from its fossils, is shown to be true mountain limestone; whilst in the contiguous extension of this field, the carboniferous strata overlie, *unconformably*, various members of the grauwacke series, with one of which, the transition limestone of Wenlock Edge, they are brought into *conformable* apposition at Lincoln Hill, on the Severn. The complicated relations of the deposits within this small and disturbed district, east of the Wrekin, are referred to the protrusion of basalt and green-stone, which occasionally tilt the strata at high angles, and sometimes occupy the seats of faults.

b. Coal-fields in the immediate vicinity of Shrewsbury.—Of these, the most important is a curvilinear zone extending from the northeastern flank of the Brythin Hills to Wellbatch, near Shrewsbury, the carboniferous strata reposing on the inclined edges of the grauwacke rocks, and dipping towards a common centre beneath the new red sandstone. Detached portions of the same zone are again found at Sutton and at Uffington: and they also follow the sinuous outline of grauwacke on the northern flanks of the Longmynd and Caer Caradoc. At Pitchford, the whole carboniferous series is represented by a bituminous breccia of a few feet in thickness.

Three thin beds of coal are, for the most part, observable, and the deposit is distinguished by an included band of limestone, similar in mineral aspect to the lacustrine limestones of Central France, and containing minute shells referrible to *freshwater* genera. The vegetable remains of the associated shales are chiefly analogous to the plants of other coal-basins; but those of Le Botwood are rich in the new species, *Neuropteris cordata*, whilst the shale of Pontesbury has offered a beautiful example of *Pecopteris blechnoides* in fructification.

After demonstrating the slight commercial value which can be attached to the thin deposits of this age, the author speculates on the probable importance of the outer zone or Pontesbury-field, which he presumes may expand to a great thickness in its passage beneath the new red sandstone of N. Shropshire and Cheshire.

c. Coal fields in the Clee Hills.—These fields are thrown up to considerable heights above the adjoining country of old red sandstone, both in the Brown Clee, and the Titterstone Clee Hills, the coal being for the most part covered with basalt.

The Brown Clee is distinguished by two tabular summits of black basalt (Jewstone), the highest of which is 1806 feet above the sea. The coal-bearing strata have for their base a hard sandstone occasionally conglomeritic, the equivalent of the millstone grit. On three sides of this ridge, these very thin and poor coal measures repose on old red sandstone, which to the west is a coarse conglomerate; but on the fourth or south-eastern side, there is interposed between the old red and the lower coal grits, a thin zone of limestone, which the author, therefore, refers to the age of the mountain limestone. Several faults are mentioned as traversing this coal tract from S.W. to N.E., one of which has been the source of eruption of much basaltic matter.

The Titterstone Clee Hill is next described; and details are presented of those parts only which were unnoticed in Mr. R. Wright's memoir. The most important of these relate to the Knowlbury field, which, from its juxta-position to the larger field of Coalbrook, the author terms a parasitic basin, and shows that it contains five seams of coal and some bands of ironstone. The strata around the verge of this basin are highly inclined, the dip diminishing as it approaches towards a common centre. Sections across this small basin exhibit considerable faults, which always occur as upcasts towards the higher sides of the hills, where the basaltic matter has found vent: coal, included between two of these upcasts, is described as much thickened and in the state of cannel coal.

Many fossil plants of new species from the Knowlbury and Gutter Works are described by Professor Lindley. Recent investigations of Mr. Lewis are mentioned, which prove the existence of a central dyke or funnel of basalt, thereby confirming the opinion formerly expressed by Mr. Bakewell; and a complete transverse section of these hills shows, that some beds of coal have been carried up on the top of the basalt, and that this rock has flowed laterally so as also to overlie the coal. Although these coal measures rest, in the greater part of their circumference, on the old red sandstone, the existence of an interpolated band of true mountain limestone is pointed out, which from the thickness of a few feet near Bennett's-end, extends to a maximum development of about 60 feet beneath one part of the Cornbrook field, where it contains subordinate beds of fine oolite, marl of various colours, abundance of characteristic organic remains, and is much contorted and dislocated.

At Orelton, near the north-eastern extremity of this range, and thus connecting it with the more eastern coal-fields, is a tortuous range of oolitic mountain limestone, which rests upon the old red sandstone and, exhibiting some extraordinary fractures and dislocations, passes beneath the unproductive or lower coal grits.

3. *Old red sandstone.*—In the vast formation comprehended under the term old red sandstone, the author includes all the red or green marls, conglomerates, sandstones, limestones, or flagstones, the youngest or highest beds of which pass immediately beneath the mountain limestone or carboniferous strata, and the lowest overlie and graduate into, the superior members of the grauwacke series. The author gives a geographical sketch of the western side of the great trough in which this formation is deposited in Shropshire, Herefordshire, and Brecknockshire; the prevailing strike of which is shown to be from N.E. to S.W., and the usual inclination to be to the S.E. The upper beds of the formation, near the Brown Clee,

and partially near the Titterstone Clee Hills, expose a thin band of conglomerate; then follow, in descending order, red or green marls, with two or more zones of impure limestone called cornstone. To these succeed micaceous flagstones and thin-bedded building stones, with other strata of marls and cornstone. Massive varieties of concretionary limestone, termed ball-stones, range along the western foot of the Brown Clee. They are sometimes from 18 to 20 feet thick, and are very dissimilar in quality and in appearance from the thin and conglomeritic bands of the rock. Alternations of red and green marls again succeed beneath the cornstones, and the base of the whole formation is usually marked, particularly in its course from Kington to Caermarthenshire, by highly micaceous greenish and reddish tile stones associated with marl. Thick-bedded, fine-grained building stones of excellent quality are worked near Hay in Herefordshire, overlying the tile stone division. No workable seam of coal has ever been discovered in the old red sandstone. Dr. Lloyd has recently discovered near Leominster and Ludlow, in the central and calcareous sandstone beds of the formation, fossils which are chiefly referrible to undescribed species of the family of Trilobites, and with them a few fragments of plants apparently terrestrial.

An expansion upon a large scale is pointed out in the old red sandstone, which, from a narrow tongue, is described as extending all over the forest of Mynidd Eppint, on the western side of which it reposes conformably and at high angles, upon the uppermost strata of grauwacke, at the chief escarpment of that rock. Many transverse sections from the grauwacke formations to the edge of the Glamorganshire coal-basin are given by the author, and they exhibit a perfect conformability between the upper beds of the old red and the lower members of the mountain limestone, as well as a gradual passage from the old red into the grauwacke. He, however, insists that there are no two formations of the English series which can be better separated from each other for purposes of geological illustration, than the old red sandstone and the uppermost grauwacke; the former being as poor as the latter is rich in organic remains, whilst the colours and mineral characters of the two formations are also very distinct. The maximum thickness of the formation is not easily defined with accuracy, but the author has no hesitation in saying that it exceeds 4000 feet.

In the latitudes of Llandovery and Llandilo, the whole formation is thrown so much on edge, that it necessarily occupies a very small superficial breadth, whilst the very slight inclination and the undulation of the beds in Herefordshire and Brecknockshire account for its vast expansion in these counties.

Detached tracts covered with this formation are pointed out as occurring far within the frontier of the grauwacke rocks; and they are considered to be true basins of elevation which have been formed on the western sides of certain anticlinal lines, along which the inferior sediments have a reversed dip.

April 17th.—The second part of a memoir, commenced on the 27th of March, and entitled, "On the sedimentary deposits which occupy the western parts of Shropshire and Herefordshire, and are prolonged from N.E. to S.W., through Radnorshire, Brecknockshire, and Caermarthenshire, with descriptions of the accompanying rocks of intrusive or igneous characters," by Roderick Impey Murchison, Esq. F.G.S. F.R.S. &c., was read.

In this part of the memoir the author separates into distinct formations by the evidences of fossils and the order of superposition, the upper portions of those vast sedimentary accumulations, which have hitherto been known only under the common terms of transition rocks, and grauwacke. Commencing at the base of the old red sandstone, which formation he had described in the previous part of the memoir, he proceeds to give an account of the underlying deposits as they succeed to each other in descending order in Shropshire and Herefordshire.

I. *Upper Ludlow Rock.*—Equivalent, Grauwacke Sandstone of Tortworth, &c.

This group, so named because the Castle of Ludlow stands upon it, is as eminently characterized by the presence of organic remains as the old red sandstone is by their deficiency. Amid a profusion of fossils, the upper beds are characterized throughout the whole range of the formation by two species of Strophomena or Leptæna, an Orbicula, a plicated Terebratula, &c. all of undescribed species. The middle beds contain many species of Orthocerata; Serpulæ? of great size, &c.; and the lower strata are charged with a profusion of small Terebratulæ having a gryphoid form. Trilobites of the genera Homonolotus andCalymene occur. The group has a maximum thickness of about 1000 feet, is for the most part a thin-bedded sandstone, often highly calcareous, and at other times argillaceous, and in Shropshire frequently occupies distinct lofty ridges interposed conformably between the old red sandstone and the inferior limestone.

II. *Wenlock Limestone.*—Equivalents, Dudley Limestone, Transition Limestone, &c.

On the banks of the Severn near Wenlock, and in the Wenlock Edge, this group is particularly abundant in corals and Encrinites, nearly all the species of which, as well as of certain Mollusca, are found in the well-known limestone of Dudley. The exact position, therefore, which the latter occupies in the geological series of England, is thus for the first time determined.

The upper beds rising from beneath the Ludlow rock are thin-bedded and lenticular, and the lower beds in Wenlock Edge contain many concretions of very great size and of highly crystalline structure. Throughout its course in the district included between the rivers Oney and Lug, this limestone is chiefly characterized by the abundance of one species of Pentamerus, and at Aymestrey it is rich in that and other fossils*. 100 feet are considered to be about the thickness of this calcareous zone.

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* The author cannot allude to the fossils of this district without expressing his deep obligation to the Rev. T. Lewis of Aymestrey, whose unceasing researches have contributed very essentially towards the zoological illustration of this memoir.

III. *Lower Ludlow Rock.*—Equivalent, "Die Earth."

This group is chiefly made up of incoherent, grayish, argillaceous schist, seldom micaceous. The higher strata are in some places charged with many Orthocerata of new and undescribed species, Lituites, *Asaphus caudatus*, &c. Other beds are locally distinguished by concretions of argillaceous limestone, formed around corals and other organic bodies; and towards the base of this deposit a thin calcareous zone is observable in Shropshire, containing the *Pentamerus lævis* and a new species of that bivalve, both differing from the species noted in the overlying group 2. The thickness is supposed to exceed 2000 feet.

The dislocations and faults on the Severn are described, by which this unproductive shale or "Die Earth" is brought in one point into unconformable contact with, and in another passes conformably beneath, the coal fields of Madeley and Brosely.

IV. *Shelly Sandstones.*—Equivalent, ———?

Red and green colours predominate in these sandstones, although purple and white are also frequently observed; by which characters, as well as by the nature of the stone and the specific differences in the organic remains, this formation is clearly distinguished from any of the overlying groups. Associated with the sandy strata are calcareous bands, almost made up of Productæ, Leptænæ, and Spiriferi, with crinoidal remains, all differing from those in the superior deposits. In Shropshire this formation rises at low angles from the valleys of lower Ludlow rock or Die Earth, and occupies separate ridges on the south-eastern flanks of the Wrekin and the Caer Caradoc. By a rough estimate 1500 to 1800 feet are assigned to the depth of the deposit.

V. "*Black Trilobite Flagstone,*" *&c.*—Equivalent, ———?

The prevailing Trilobite in this formation is the large *Asaphus Buchii*, which with the other associated species is never seen in any of the overlying groups. In the mountain called the Longmynd, this flagstone is made up of black schists, hard and dark-coloured grauwacke sandstone, &c., in which Trilobites have not yet been observed, although they are abundantly characteristic of the same zone in its prolongation through Radnor, Brecknock, and Caermarthen shires, where these fossils occur in black limestone and calcareous flagstone and grit. The thickness of this formation probably exceeds that of any one of the superior groups.

VI. *Red Conglomerate, Sandstone, and Slaty Schist.*

This is a vast deposit of several thousand feet in thickness, consisting of very coarse, quartzose conglomerates, which alternate with some schistose beds and much purple-coloured sandstone (Compound Sandstone of Townson), the strata of which in Haughmond, Pulverbatch, and Linley Hills, Shropshire, are highly inclined or vertical, in conformity with those of the preceding formation. No organic remains have been observed, by which, and by its very peculiar mineral structure, this formation is shown to be entirely distinct from the preceding groups.

The above six deposits are all exhibited in Shropshire, trending from N.E. to S.W., and occupying distinct ridges and valleys. In their further prolongation to the S.W. the upper Ludlow rock is uniformly persistent. Strata lithologically similar and containing the same fossils are found invariably to rise from beneath the old red sandstone in the counties of Hereford, Radnor, Brecknock, and Caermarthen; sometimes at very low angles of inclination, while at others, as in the promontories near Ludlow and Brecon, they are thrown up into saddles, and at the south-western limit of Brecknock and Caermarthen shires they are vertical or very highly inclined.

The second deposit, or Wenlock (Dudley) limestone, thins out a little to the S.W. of Aymestrey, and the groups 1 and 3 being brought together, generally occupy the same lofty escarpment in their course through S. Wales. Hence the author suggests the term Ludlow formation (the upper and lower Ludlow rock being subordinate members), as applicable to all the higher portion of this series which has a tripartite character in Salop and Hereford, due to the interpolation of the Wenlock and Aymestrey limestone.

The deposits 4, 5, and 6, are three separate formations, entirely differing from each other, and from the Ludlow formation, in their characters, mineral and fossil, and in the distinctness of their physical demarkations. They are not, however, to be traced continuously in their course from Shropshire on the N.E. to Caermarthenshire on the S.W.; though they reappear at intervals on that strike, preserving their relative places in the geological series.

In those districts where parallel ridges of all these formations are brought to day within a zone of small breadth, rocks of trappean or igneous origin are usual accompaniments, as in the neighbourhoods of the Wrekin and Caer Caradoc, in Shropshire; and again, after a long interval, in the environs of Old Radnor, Builth and Llandegley. In the intervening and featureless tracts of Clun, Knuckless and Radnor forests, where such intrusive rocks are absent, the Ludlow formation alone is spread out in undulating masses, and upon its surface are frequently found detached and elevated basins of old red sandstone.

The heights of the different groups above the sea-level vary from 500 to 2000 feet.

The author reserves for the third part of his memoir, which he proposes to communicate on a future occasion, the description of the numerous trappean and porphyritic rocks, which, in penetrating through these grauwacke deposits, have produced changes in their mineral aspect and structure. On that occasion the question of the parallelism of these sedimentary groups will be reviewed in reference to the direction of the outbursts of rocks of igneous origin. The quartz rock on the flanks of the Wrekin and Caer Caradoc, and also in the singular ridge of the Stiper Stones, will be described under the head of "Altered Rocks." The relations of the formations on the eastern side of Herefordshire will also be explained, with the view of determining whether deposits of the same age and character rise from beneath the old red sandstone in the flanks of the Malvern Hills, &c., as have been shown to exist on the opposite or western side of the great field of old red sandstone.

May 1.—A paper was first read, entitled, "Notice of a Machine for regulating high temperatures, invented by the late Sir James Hall, Bart., F.G.S.," and drawn up by Captain Basil Hall, R.N., F.G.S. &c.

A letter was afterwards read from Mr. Telfair to Sir Alexander Johnstone, V.P.R.A.S., accompanying a specimen of recent conglomerate rock, from the Island of Madagascar, containing fragments of a tusk, and part of a molar tooth of a hippopotamus; and communicated by Roderick Impey Murchison, Esq. F.G.S.

ROYAL ASTRONOMICAL SOCIETY.

May 10.—The following communications were read:—

On the Latitude and Longitude of the Cape Observatory. By Mr. Henderson.

In this paper Mr. Henderson assumes the geographical position of the Cape Observatory to be 33° 56′ 3″ south latitude; 1h 13m 56s east longitude from Greenwich; the former being determined from his own observations, and the latter from those of Mr. Fallows compared by Mr. Henderson with the corresponding observations made at European observatories. From the determination of the longitude of the Cape Observatory here stated, compared with observations made by Lieut. Meadows and by Capt. Owen, Mr. Henderson also finds that the longitudes of the two Capes which inclose False Bay may be stated as follows:—

Cape of Good Hope	18°	29′	0″	
False Cape	18	50	30	east of Greenwich.

"This longitude of the Cape Point," Mr. Henderson observes, "differs only one mile from that given by La Caille (16° 10′ east of Paris—*Memoirs of Academy of Sciences* for 1751, p. 425); and it is but justice to the memory of that distinguished astronomer to remark, that the lapse of eighty years, and the superior means which the present state of the science affords, have not been able to produce any improvement in the geographical position of this part of the world, as determined by him."

Positions of Stars near the South Pole, from Observations made at the Observatory at the Cape of Good Hope; also by Mr. Henderson.

There being no conspicuous star near the South Pole, which can be observed in the day-time, even with the most powerful meridian telescopes that are used in observatories, it becomes necessary, in the southern hemisphere, for the purpose of determining the polar positions of astronomical instruments, to have recourse to stars of less magnitude than those which are observed for that purpose in the northern hemisphere, and even to increase their number, in order that one or more of them may pass the meridian at a convenient time of the night. Since Mr. Henderson's arrival at the Cape Observatory, in April 1832, seven circumpolar stars have been frequently observed with Dollond's 10-feet transit telescope, and Jones's 6-feet mural circle, for the purpose of determining the positions of those instruments with regard to the Pole. Of the stars whose places have been thus determined, believing that their positions may be of considerable use to astronomers, Mr. Henderson now communicates a Catalogue, exhibiting their mean positions reduced to the beginning of 1832 (fictitious year of the Astronomical Society's Catalogue). This Catalogue, under the title of "Positions of Stars near the South Pole for the beginning of 1832," is given in the "Monthly Notices" of the Society for May.

Various Observations made at the Cape Observatory, by Mr. Henderson, Mr. Meadows, and Mr. Fayrer: viz. observed right ascensions of the moon and stars 1832, April 10 to October 17, inclusive; observations of the moon and stars, made with the mural circle, for the determination of the moon's parallax; 1832, July 20, to January 2, 1833,—of Mars and stars, made with the mural circle, for the determination of the parallax of Mars: eclipses of Jupiter's satellite, 1832, April 12 to October 19; occultations of planets and fixed stars by the moon, 1832, August 10 to October 31; and the transit of Mercury, May 4 and 5, 1832.

A paper on the Solar Parallax. By Mr. Shires.

On the Determination of the true Heights of distant Mountains from their apparent elevations above the sea. By G. Blackburne, B.A.

The object of this investigation is to furnish a convenient formula for the computation of the terrestrial refraction in terms of the distance of the observer from the object, and the apparent elevation; and secondly, for the computation of the true height of the object itself.

June 14.—The following communications were read; some of which we shall further notice in our next Number.

I. Observations of Biela's Comet, from Nov. 18, 1832, to J[illegible]33, 1833. By T. Henderson, Esq., Astronomer at the Cape of Good Hope.

II. Various Observations made at the Observatory of the Cape of Good Hope. Communicated by T. Henderson, Esq.

This series of observations, made at the Cape Observatory by Messrs. Henderson and Meadows, is a continuation of those already communicated, as noticed above.

III. Observations of *Mars*, and Stars observed with *Mars*, near the Opposition, between Oct. 14, and Dec. 22, 1832, at the Observatory, St. Helena. By Lieut. Johnson.

IV. Observations of the Magnitude, Colour, and Brightness of Stars in the Southern Hemisphere. By James Dunlop, Esq., Astronomer at Paramatta.

V. Observations of several Meridian Transits of the Moon. By the Rev. M. Ward.

VI. Observations of Occultations and Eclipses made in the year 1832, at Biggleswade, with the Wollaston Telescope. By T. Maclear, Esq.

VII. Description of a small Observatory erected at Biggleswade. By T. Maclear, Esq.

VIII. An Approximate Method of finding the Latitude from two Altitudes observed near the Meridian, with the interval of time. By Lieut. Raper.

IX. Notice of the Elliptic Orbit of ξ *Boötis*, with a further Approximation to the Orbit of γ *Virginis*. By Sir John Herschel.

X. On the Adoption of a Standard of Optical Power by Observers. By the Rev. W. R. Dawes.

XI. Description of a Double Reflecting Circle, and of a Quadruple Reflecting Sextant, made by Mr. Jones, of Charing Cross, under the direction of Captain W. F. Owen, R.N., and presented by the latter to the Society.

XII. Transits of the Moon with Moon-culminating Stars observed at Cambridge Observatory, in the months of March, April, May, and June, 1833.

Agreeably to the intention expressed in vol. ii. p. 476, we now return to some prior meetings of the Society, during the present year, which have not hitherto been noticed in the Philosophical Magazine, in order to complete our calendar of its proceedings.

February 8.—At the Annual General Meeting holden this day, being the Thirteenth, the Report of the Council was read, the Gold Medal was presented to Professor Airy, for his paper "On an irregularity of long period in the motions of the Earth and Venus," published in the Philosophical Transactions for 1832, (of which an abstract was given in Phil. Mag. and Annals, N.S., vol. xi. p. 117: see also Lond. and Edinb. Phil. Mag., vol. i., p. 61.) and the following Fellows were elected the Council and Officers for the ensuing year: viz.

President: Francis Baily, Esq., V.P.R.S.—*Vice Presidents*: George Biddel[illegible] Esq., M.A., Plumian Prof. Ast. University of Cambridge; Captain [illegible] Beaufort, R.N. F.R.S.; Davies Gilbert, Esq., F.R.S.; John William [illegible]k, Esq., M.A. V.P. and Treas. R.S.—*Treasurer*: Jo[illegible]e, Esq., LL.D.—*Secretaries*: George Bishop, Esq.; Augustus De [illegible]an, Esq.;—*Foreign Secretary*: Captain W. H. Smyth, R.N. F.R.S.—*Council*: Bryan Donkin, Esq.; Lieut. Thomas Drummond, R.E.; Thomas Jones, Esq.; Rear Admiral Sir Edward Owen, K.C.B.; Lieut. Henry Raper, R.N.; Edward Riddle, Esq.; Rev. Richard Sheepshanks, M.A. F.R.S.; Robert Snow, Esq., B.A.; Lieutenant William S. Stratford, R.N. F.R.S; Thomas Turner, Esq. M.A.

April 12.—(continued from vol. ii. p. 475.) Of the researches detailed and discussed in Professor Airy's paper "On the Mass of Jupiter" read at this meeting, we have already given a summary account, at p. 314 of our last volume. Some particulars of the numerical errors discovered by the author in the *Mécanique Céleste*, and his final statement and estimate of the value of his own results, we now subjoin:

"I had at first no intention of correcting, or even examining the numbers of the *Mécanique Céleste*; but an examination simply for the verification of the typographical accuracy, led me to doubt the correctness of several of the numbers in page 94 of the fourth volume, and of those which depend on them. On further examination, I found that the variable terms, both of the radius vector and longitude, as far as I examined, were nearly correct; but that in the constant parts of the radius vector some terms were wrong in sign, one erroneous in magnitude, and one omitted. I corrected these, and then proceeded in the construction of my tables."

"In the first instance, however, to which I applied the calculations, I found, to my great astonishment, that, at the time when I had observed the satellite at extreme elongation, the tables represented it as between the earth and *Jupiter*. I satisfied myself that this was owing to no error in my own calculations, and I then compared Laplace's numbers with those of Halley and Delambre. From the tables of the former I could only infer that there was some very strange error in Laplace's epoch for 1750; from the mean conjunctions of the latter I could calculate its quantity. It will scarcely be believed, that the epoch of mean longitude in page 139 of the fourth volume of the *Mécanique Céleste* is erroneous to the amount of nearly one third of the circumference, and that this is not a typographical error, as several numbers are combined with it by addition and subtraction in the pages following it, and all correctly. I confess that my faith in the *numerical* results of the *Mécanique Céleste* has been very much weakened by these discoveries."

"At the same time, I am far from imputing any negligence to the author of that great work; for I know too well the difficulty of insuring correction in long calculations from complex formulæ: and in some of the fundamental numbers of the present computations I was saved from error only by Mr. Lubbock's re-calculations."

A list of the errata in the *Mécanique Céleste* is appended to the memoir.

Returning to Professor Airy's own researche[illegible] s[illegible]

"After applying two small corrections, one f[illegible]e clo[illegible] the observations, and the other for obtaining t[illegible] mass o[illegible] Jovial system, Professor Airy concludes, that we have for [illegible]3, mass of the Jovial system 6·9793486, and for the mass $\frac{1}{1048{\cdot}70}$ [illegible]

"In conclusion I may remark, that I do not think this value liable to any serious error, except from a possible error in the inclination of the orbit of the fourth satellite. The effect of the greatest error of the instrument in azimuth (0′·5) that any observations have shown, acting at the greatest disadvantage, would not alter the denomination of the fraction by 0·2. But the observations used here have been made when *Jupiter* was in the most unfavourable of all positions, both from the effect of such errors, and from the smallness of the difference of right ascension, which is the immediate subject of the measures. The oppositions of 1834-5 and 1836 will be very much more favourable."

"I am far from regarding the question as definitively settled by these measures; and I offer these results to the Society as only the first fruits of my investigations. Should the regular business of the Observatory, and my other employments, leave me sufficient leisure, I hope to add considerably to their number. The necessary tables and forms of calculations are prepared; some theoretical and numerical difficulties have been surmounted; and the labour of calculating future observations will therefore be very much less than that of calculating the first."

PHILOSOPHICAL SOCIETY OF CAMBRIDGE.

May 20.—The following communications were read:—

On the Attraction of Spheroids, by G. Green, Esq In this paper the author presents certain analytical formulæ, in reference to triple integrals of a more general form than those offered in the attractions of spheroids of arbitrary form and density, and applies them to the problem of the attractions of ellipsoids, so as to comprise the actions on points, internal and external, in a common process, by the addition of a positive quantity, under the radical sign in the expression, for the reciprocal distance between the point acted on and any point of the ellipsoid, which quantity is afterwards made to vanish.

On the Determination of the Vibratory Motion of Elastic Fluids in Tubes of definite Length, by W. Hopkins, Esq. of St. Peter's College. The author described a series of experiments made by him with the view of subjecting to an experimental test the different solutions which have been given of this problem. The intensity of the vibrations in any part of the tube are indicated to the eye by the motion which those vibrations excite in a delicate membrane, sprinkled with light sand, and suspended in the tube. The positions of the nodal points, thus determined with great accuracy, are not such as accord with any solution of the problem hitherto given; but it was shown how all the observed phænomena are accounted for by the assumption of certain physical conditions more general than those assumed by previous writers.

An experiment was also exhibited by Mr. Hopkins, showing the effect [illegible] interference of two aërial undulations proceeding in the [illegible] The ends of two equal tubes, branching off from one common tube, placed close to two ventral segments of a vibra-[illegible] te, by which the vibrations are excited in the branch tubes [illegible] ere in the one with which they communicate. If the vibrations proceeding from the two ventral segments be in the same phase, the resulting vibration is one of great intensity; but if they are in opposite phases no sensible vibration results from them. The intensity of the vibration is indicated, as above mentioned, by a membrane which may be stretched over the mouth of the tube.

XLII. *Intelligence and Miscellaneous Articles.*

COMPOUNDS OF CHROMIC ACID WITH METALLIC CHLORIDES.

M. E. PELIGOT has formed several compounds of chromic acid with metallic chlorides. The bichromate of chloride of potassium is easily and œconomically produced by boiling a mixed solution of bichromate of potash and muriatic acid for some time; on cooling, a quantity of the salt in question crystallizes, in proportion to the quantities of ingredients employed.

According to M. Peligot the theory of the formation of this salt is as follows: the muriatic acid at first acts upon the potash of the bichromate, and forms chloride of potassium, and water, and at the same time free chromic acid is developed. If, when all the potash has been converted into chloride of potassium and water, the solution is allowed to cool, it soon deposits large crystals of bichromate of chloride of potassium; so that all the bichromate of potash is converted into water and bichromate of chloride of potassium before the muriatic acid, supposed to be in excess, produces protochloride of chrome, which only happens by the decomposition of the salt which is formed.

The atomic reaction is thus given:—

Atoms employed.		Atoms produced.	
2 atoms chromic acid	1304	2 atoms chromic acid......	1304
1 atom potash......	589	1 atom chloride of potassium	931
2 atoms muriatic acid	454	1 atom water	112
	2347		2347

The principal properties of this salt are, that it crystallizes very easily; the crystals are right prisms with rhombic bases; their colour is similar to that of bichromate of potash; they do not deliquesce. The action of water on this salt is remarkable: when a crystal is moistened with it, it becomes white and opake; and if the solution be suffered to evaporate spontaneously, no crystals of the salt in question are obtained; but merely those of bichromate of potash. This latter salt is, indeed, produced whether the liquor be concentrated by heat, or exposed to spontaneous evaporation. It appears, then, that water is decomposed [illegible] chloride of potassium: its hydrogen combin[illegible] form muriatic acid which becomes free, while [illegible] the potassium.

This reaction produces precisely the products employ[illegible] the formation of the bichromate of the chloride, and the [illegible] the operation will be shown by transposing the atoms above given.

Atoms employed.		Atoms produced.	
2 atoms chromic acid	1304	2 atoms chromic acid . .	1304
1 atom chloride of potassium	931	1 atom potash	589
1 atom water	112	2 atoms muriatic acid . .	454
	2347		2347

By the addition of a sufficient quantity of muriatic acid, to correct the oxidizing action of the water, the bichromate of the chloide is reproduced. On account of this action of the water, it is impossible to purify this salt by crystallization: it is requisite to press it between folds of blotting paper: it almost always contains a slight excess of chlorine.

The most œconomical process for obtaining this salt has been already stated; it may also be obtained, and in a more direct manner, by mixing 2 atoms of chromic acid with 1 atom of chloride of potassium, provided the solution be rendered acid by muriatic acid. It may be procured also by treating bichloride of chrome with water saturated with chloride of potassium; and in this case all the conditions favourable to its formation are fulfilled, for the water converts the bichloride of chrome into chromic and muriatic acid. If neutral chromate of potash be used instead of the bichromate, the crystals obtained are mixed with crystals of chloride of potassium.

The following process of analysis was preferred and employed on account of its simplicity:—A portion of the salt, dried between the folds of blotting paper, was dissolved in distilled water acidified by nitric acid, and nitrate of silver was then gradually added, and the chloride obtained was washed, dried and weighed. The liquor, after the separation of the chloride of silver, was boiled with sulphurous acid; this converts the chromic acid into protoxide of chrome, and is itself converted into sulphuric acid: ammonia then precipitates protoxide of chrome by long boiling. The potash was then converted into sulphate by means of sulphuric acid, and its quantity thus determined.

The bichromate of muriate of ammonia resembles that of chloride of potassium in appearance and crystalline form, but it is much more soluble in water. By analysis it yielded—

Chromic acid	65·5
Muriatic acid	23·5
Ammonia	10·8
	99·8

[illegible]ot found that the bichromates of the chlorides of sodium, [illegible] magnesium were deliquescent: he was unable to pro- [illegible] of the chlorides of strontium and barium, on [illegible]itation of their salts from water by the muri- [illegible] *Chim. et de Phys.* lii. 267.

ON FOSSIL-BONE CAVERNS.

[illegible]urnal, jun., of Narbonne, concludes from a great variety of facts and observations, that,

1st, The bones buried in caverns have been introduced in several different manners.

2ndly, The species buried vary in different localities; and this difference depends either upon the period of deposition or the geographical position of the cavern.

3rdly, Man was contemporary with the lost species of animals which are found buried in the mud of the caverns; and these animals being regarded by all naturalists as fossil, man therefore exists in the fossil state.

4thly, The mud and pebbles found in the bone caves were not introduced by any sudden and temporary cause, as by a deluge, but on the contrary, almost always slowly, and in several different modes.

5thly, The attentive examination of the pebbles contained in the mud proves evidently that they have been introduced from neighbouring places, and consequently, that the cause which conveyed them was entirely local.

6thly, The period during which the bone caves were formed was extremely long.

7thly, In certain caverns, the mud, bones, and pebbles have been simultaneously introduced; in others, on the contrary, the mud has been deposited after the bones.

The bones of the following animals have been found in the caverns of the South of France:—

Elephant.
Rhinoceros.
Wild Boar.
Horse.
Ox, two species.
Stag, five species.
Antelope, very large.
Chamois.
Hyæna, fossil.
Hyæna, striped.
Hyæna, brown.
Dog, two species.
Goat.
Sheep.
Bear, at least two species.
Badger.
Tiger.
Lion.
Leopard.
Lynx.
Wolf.
Fox.
Weasel.
Pole-cat.
Hare.
Rabbit.
Lagomys.
Campagnol.
Birds, several species.
Land Tortoise.
Lizard (*Lacerta ocellata*).
Snake, size of the *Coluber Natrix*.

The only difference which exists between the caves of England and Germany, compared with those of the South of France, [illegible] in England the hippopotamus has been found, and at [illegible] Westphalia, the glutton.—*Ann. de Chim. et de Phys.* lii[illegible]

ON THE VISIBILITY OF STARS [illegible]

Sir John Herschel, in his popular Treatise [illegible] notices the fact of stars being distinctly vis[illegible] light by telescopes, their brightness being [illegible] power of the instrument; but, which is unusual wi[illegible] author, he omits to explain the reason.

If the following be not the true reason, perhaps correspon[illegible] of the Philosophical Magazine will be kind enough to correct me.

The rays of day-light which enter the object-glass of a telescope must, from our proximity to the points of their reflection, be much more divergent than the rays of a distant star; consequently none of the former rays can be converged to so short a focus as the latter. Hence, if a telescope be so adjusted as to bring the "parallel" rays from a distant star to a focus, none of the divergent rays from the nearer atmosphere can be condensed at the same or so short a focus, and the atmospheric light within the tube cannot much interfere with the concentrated light of the star. As we cannot, while looking through a telescope at a distant ship, see the flame of a candle, or cross wires, held immediately before the object-glass, by reason of the nearer flame or wires' requiring a longer focus than the distant ship, so for the same reason we cannot, while looking through a telescope at a distant star, see the nearer light of the atmosphere which partially intercepts it. In either case that which intervenes produces no other effect than a partial obscuration of the distant object.

Sir J. Herschel notices also the fact of bright stars in the zenith being visible in the day-time, even by the naked eye, from the bottoms of deep narrow pits, or through the shafts of chimneys. May not this be owing to the divergent rays of the atmosphere spreading and losing themselves against the non-reflecting sides of the pit or shaft, so that few or none of them ever reach the bottom; while, on the contrary, all the rays from the star which enter the vertical pit proceed to its bottom without the least sensible divergence?

Redruth, July 13, 1833. RD. EDMONDS Jun.

LEYBOURN'S MATHEMATICAL REPOSITORY.

The twenty-fourth Number of this valuable Work is just published, and contains the following articles:—

1. *Nugæ*, or Constructions to represent approximately the length of the circumference of a Mile.

2. Solutions of the twenty Mathematical Questions proposed in No. 22; by various Contributors.

3. New Solution to Question 531 (relating to the Αρβηλον of Archimedes), and some collateral properties of the figure; by T. S. [illegible] Esq. F.R.S. Lond. & Ed.

[illegible] of the late Sir John Leslie, K.H.

[illegible]g to the Mathematical Parts of the Philosophi- [illegible] Transactions of the Royal Society of Edin- [illegible] of the Cambridge Philosophical Society, and [illegible] *Polytechnique*.

[illegible]e Problems; by James Cunliffe, Esq.

[illegible] to the Lunar Theory; by the Rev. Brice Bron- [illegible]

[illegible]e Computation of Surfaces and Solids bounded by lines [illegible] passing through points, by means of the coordinates of those points; by Mr. Woolhouse.

9. New Researches in Spherical Trigonometry; by T. S. Davies, Esq.

10. On the Deviation of two Curves, in answer to a note of Mr. Woolhouse, inserted in the last Number; by T. S. Davies, Esq.

11. On a partial Differential Equation; by W. Sutcliffe, Esq. A.M., Bath.

12. On the Figure of a homogeneous Mass of Fluid revolving about an axis with a given uniform Velocity; by the Rev. Brice Bronwin.

13. Translation of Dr. Matthew Stewart's Extension of Prop. 4. Lib. iv. of the Mathematical Collections of Pappus (concluded).

14. Certain Properties of Plane Triangles not generally known; by C. F. A. Jacobi.

15. Twenty new Mathematical Questions for solution in No. 26; by various Contributors.

Days of Month, 1833.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost. 8½ A.M.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.							
July 1	29·873	29·831	29·916	29·819	29·19	66	40	58	48	58	W.	NW.	W.	0·20	0·280	...
○ 2	29·998	29·919	29·969	29·966	29·35	76	45	60	48	58	SW.	NW.	W.	...	...	...
3	30·121	30·014	30·116	29·966	29·48	69	44	62	47	58	W.	W.	NW.	...	...	...
4	30·178	30·168	30·113	30·033	29·12	75	51	62	54	65	W.	W.	calm	...	...	...
5	30·167	29·997	30·013	29·904	29·54	76	49	66	55	68·5	SW.	SE.	calm	...	...	...
6	29·857	29·656	29·657	29·560	29·28	77	50	63	55	73	SE.	NW.	SE.	·01	·200	...
7	29·767	29·612	29·660	29·560	29·02	68	55	64	50	73	S.	NW.	calm	·20	...	...
8	30·022	29·808	29·954	29·860	29·20	71	52	66	51	68·5	W.	NW.	E.	·24	...	...
9	30·108	30·080	30·107	30·104	29·51	73	45	68	55	58	N.	NW.	NE.	...	...	...
☾ 10	30 031	29·959	30·104	30·004	29·46	76	51	69	56	54	NW.	NW.	calm	...	...	...
11	29·992	29·894	30·004	30·004	29·36	69	53	64	55	65	N.	NW.	E.	...	...	...
12	29·899	29·852	29·857	29·857	29·33	61	50	60	53	57·5	NE.	NW.	calm	...	·125	...
13	29·921	29·854	29·910	29·904	29·43	64	47	65	55	58	SW.	NW.	NE.	...	...	...
14	30·002	29·958	29 907	29·854	29·46	73	54	63	53	63	SW.	SW.	calm	...	·920	...
15	30·131	30·006	30·054	29·910	29·47	75	45	65	53	65	NE.	SE.	calm	...	...	...
16	30·211	30·194	30·154	30·110	29·61	79	50	68	53	64	E.	NE.	calm	...	...	...
● 17	30·220	30·194	30·154	30·148	29 55	84	55	70	55	66	NW.	NE.	calm	...	...	...
18	30·162	30·011	30·148	30·048	29·44	81	57	70	54	72	W.	SE.	calm	·02	...	...
19	29·906	29·772	29·948	29·848	29 20	76	57	66	57	66	SW.	NW.	W.	...	·055	...
20	29·671	29·618	29·804	29·804	28 95	70	48	63	55	[illegible]	[illegible]	NW.	NW.	·18	·060	...
21	29·877	29·778	29·904	29·860	29·12	70	46	63	[illegible]	[illegible]	[illegible]	NW.	NW.	·05	...	...
22	29·835	29 697	29·857	29·798	29·17	73	54	65	[illegible]	[illegible]	[illegible]	SW.	W.	·26	·040	0·10
☽ 23	29·912	29·638	29·945	29·704	29·13	70	48	64	[illegible]	[illegible]	[illegible]	W.	W.	·40	·130	...
24	30·196	30·024	30·098	30·024	29·43	75	48	65	[illegible]	[illegible]	[illegible]	[illegible]	W.	...	...	...
25	30 327	30·281	30·210	30·198	29·68	77	45	67	[illegible]	[illegible]	[illegible]	[illegible]	W.	...	...	·30
26	30·311	30·250	30·204	30·198	29·70	80	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	W.	...	...	...
27	30·265	30·255	30·[illegible]04	30·192	29·60	86	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	W.	...	...	·12
28	30·287	30·257	30·198	30·166	29·62	82	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	...	...	·08
29	30·328	30·308	30·172	30·166	29·69	79	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	...	...
30	30·458	30·423	30·246	30 212	29·83	79	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	...	...
○ 31	30·429	30·351	30·292	30·286	29 83	79	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	...	...	...
	30·458	29·612	30·292	29·560	29·41	86	[illegible]	[illegible]	[illegible]	[illegible]				1·46	1·810	0·60

Remarks.

London. — July 1. Fine: heavy rain, and a hail shower about 7 P.M.: clear at night. 2. Fine. 3. Fine: thunder. 4, 5. Fine. 6. Very dry. 7. Cloudy: sultry with showers. 8. Heavy rain at noon: cloudy and rather windy at night. 9—11. Fine. 12. Dry haze: fine. 13. Overcast: fine. 14—19. Very fine. 20. Fine: cloudy: showery. 21. Cloudy: showery: fine. 22. Rain: clear. 23. Cloudy: rain. 24—26. Very fine. 27. Very hot: thermometer 86° in shade. 28, 29. Very hot. 30. Cloudy: fine. 31. Overcast: clear and fine.

Penzance. — July 1. Showers. 2. Fair. 3. Misty: fair. 4, 5. Fair. 6. Fair: rain. 7. Fair. 8, 9. Clear. 10, 11. Fair. 12. Rain: fair. 13. Fair. 14. Fair: rain. 15. Fair. 16, 17. Clear. 18. Clear: fair. 19, 20. Clear: showers. 21. Fair. 22. Showers: fair. 23. Rain: fair. 24—31. Clear.

Boston. — July 1—4. Fine. 5. Cloudy. 6, 7. Fine. 8—10. Cloudy. 11. Fine. 12, 13. Cloudy. 14. 15. Fine. 16, 17. Cloudy. 18. Fine. 19. Cloudy. 20. Fine. 21. Fine: rain P.M. 22. Cloudy. 23. Fine. 24. Fine: rain P.M. 25. Fine. 26, 27. Fine: rain P.M. 28, 29. Fine. 30, 31. Cloudy.

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

OCTOBER 1833.

[illegible] of a Means of preparing the Organs of Re- [illegible] considerably to extend the Time of Holding [illegible] th Remarks on its Application, in Cases in [illegible] red to enter an irrespirable Atmosphere, and [illegible] necessary to be observed in such Cases.

[illegible] DAY, Esq. D.C.L. F.R.S. &c. Fullerian [illegible] ry in the Royal Institution.

[illegible] of the Philosophical Magazine and Journal.

[illegible] men,

[illegible] are many facts which present themselves to observant men, and which, though seen by them to be curious, interesting, and new to the world, are not considered worthy of distinct publication. I have often felt this conclusion to be objectionable, and am convinced that it is better to publish such facts, and even known facts under new forms, provided it be done briefly, clearly, and with no more pretension than the phænomena fairly deserve. It is this feeling which makes me send for your acceptance or rejection an account of an effect, new to me, and to all to whom I have mentioned it, and which seems to have some valuable applications.

At one of the scientific meetings at the apartments of His Royal Highness the President of the Royal Society, whilst speaking of certain men who, by means of peculiar apparatus for breathing, could walk about at the bottom of waters, and also of the pearl fishers, Sir Graves C. Haughton described to me an observation he had made, by the application of which a man could hold his breath about twice as long as under ordinary circumstances. It is as follows:—If a person inspire deeply, he will be able immediately after to hold breath for a time, varying with his health, and also very much with the state of exertion or repose in which he may be at the instant. A man, during an active walk, may not be able to cease from breathing for more than half a minute, who, after a period of rest on a chair or in bed, may refrain for a minute or a minute and a half, or even two minutes. But if that person will prepare himself by breathing in a manner deep, hard and quick, (as he would naturally do after running,) and, ceasing that operation with his lungs full of air, then hold his breath as long as he is able, he will find that the time during which he can remain without breathing will be double, or even more than double the former, other circumstances being the same. I hope that I have here stated Sir Graves C. Haughton's communication to me correctly; at all events whilst confirming his observation by personal experience, I found [illegible]lts to be as above.

Whilst thus preparing myself, I always [illegible] feelings come on resembling in a slight degr[illegible] by breathing a small dose of nitrous oxide; s[illegible] confusion in the head are at last produced; [illegible] breathe, the feeling gradually goes off, no[illegible] from it either at the time or afterwards[illegible] breath comfortably for a minute and a [illegible] and a half, walking briskly about in the me[illegible].

Now this effect may be rendered exceedingl[illegible] There are many occasions on which a person who [illegible] breath for a minute or two minutes, might save the [illegible] another. If, in a brewer's fermenting vat, or an opened ces[illegible]pool, one man sinks senseless and helpless, from breathing the unsuspected noxious atmosphere within, another man of cool mind would by means of this mode of preparation, which requires nothing but what is always at hand, have abundant time, in most cases, to descend by the ladder or the bucket, and rescue the sufferer without any risk on his own part. If a chamber were on fire, the difference in the help which could be given to any one within it by a person thus prepared, and another who goes in, perhaps, with lungs partially exhausted, and who, if he inhale any portion of the empyreumatic vapours of the atmosphere, is stimulated to inspire more rapidly, and is therefore urged to instant retreat into fresh air, is so great, that no one who has noticed what can be done in a minute or in two minutes of time can doubt the value of the preparation under such circumstances, even though from want of practice and from hurry and alarm it may be very imperfectly made. In cases of drowning, also, a diver may find his powers of giving aid wonderfully increased by taking advantage of Sir Graves Haughton's fact.

I have myself had occasion to go more than once or twice into places with atmospheres rendered bad by carbonic acid, sulphuretted hydrogen or combustion; and I feel how much I should have valued at such times the knowledge of the fact above stated. Hoping, therefore, that it may be useful, I will add one or two precautions to be borne in mind by those who desire to apply it.

Avoid all unnecessary action; for activity exhausts the air in the lungs of its vital principle more quickly, and charges it with bad matter. Go collectedly, coolly and quietly to the spot where help is required: do no more than is needful, leaving what can be done by those who are in a safe atmosphere (as the hauling up of a senseless body, for example,) for them to do.

Take the precautions usual in cases of danger in *addition* to the one now recommended. Thus, in a case of choke-damp, as in a brewer's vat, hold the head as high as may be; in a case of fire in a room, keep it as low down as possible.

If a rope is at hand, by all means let it be fastened to the person [illegible] is giving help, that he may be succoured if he should venture [illegible] far. It is astonishing how many deaths h[illegible] in cesspools, and similar cases, for want [illegible] precaution.

[illegible] hardly needful to say, do not try to breathe the air of the place where help is required. Yet many persons fall in consequence of forgetting this precaution. If the temptation to breathe be at all given way to, the *necessity* increases, and the helper himself is greatly endangered. Resist the tendency and retreat in time.

Be careful to commence giving aid with the lungs *full* of air, not *empty*. It may seem folly to urge this precaution, but I have found so many persons who, on trying the experiment on which the whole is based, have concluded the preparation by closing the mouth and nostrils *after an expiration*, that I am sure the precaution requires to be borne in mind.

I have thought it quite needless to refer to the manner in which the preparation enables a person to increase so considerably the time during which he may suspend the operation of breathing. It consists, of course, chiefly in laying up for the time, in the cells of the lungs, a store of that vital principle which is so essential to life. Those who are not aware of the state of the air in the lungs during ordinary respiration, and its great difference from that of the atmosphere, may obtain a clearer notion from the following experiment. Fill a pint or quart jar with water over the pneumatic trough, and with a piece of tube and a forced expiration throw the air from the lungs in their *ordinary state* into the jar; it will be found that a lighted taper put into that air will be immediately extinguished.

A very curious fact connected with the time of holding the breath was observed by Mr. Brunel, jun., and has, I think, never been published. After the river had broken into the tunnel at Rotherhithe, Mr. Brunel descended with a companion (Mr. Gravatt, I think,) in a diving-bell, to examine the place: at the depth of about 30 feet of water, the bell touched the bottom of the river, and was over the hole; covering it, but too large to pass into it. Mr. Brunel, after attaching a rope to himself, inspired deeply, and sunk, or was lowered through the water, in the hole, that he might feel the frames with his feet, and gain further knowledge, if possible, of the nature of the leak. He remained so long beneath without giving any signal, that his companion, alarmed, drew him up before he desired; and then it was found that either of them could remain about twice as long under water, going into it from the diving-bell at that depth, as they could under ordinary circumstances.

This was supposed to be accounted for, [illegible] the time, by the circumstance that at the depth of 30 feet [illegible] of double pressure, and that the lungs, the[illegible] as much air as they could do under common ci[illegible] It is, however, quite evident that another advantageous circumstance must have occurred, and that the air in the lungs was also better in quality than it would have been at the surface of the river, as well as denser; for supposing the deterioration by breathing to continue the same for the same time, it is clear that every inspiration passed into the lungs twice as much pure air as would have entered under common circumstances: the injured air must, therefore, have been removed more rapidly, and the quality of that at any one time in the lungs must have risen in consequence. When to this is added the effect of double quantity, it fully accounts for the increased time of holding the breath; and had the effect of the mode of preparation now described been also added, it is probable that the time would have appeared astonishingly increased.

I am, Gentlemen, yours, &c.

M. FARADAY.

XLIV. *Some Observations on the Nature of Coal, and on the Manner in which the various Strata of the Coal-measures must probably have been deposited.* By George Fairholme, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

[illegible] submitting the following observations to your notice, and [illegible] the readers of your very able and instructive [illegible] should you consider them worthy of a place [illegible] m chiefly induced by the growing interest now [illegible]ken in geological subjects, and by the high [illegible] must always attach to such new facts, when [illegible]t, as bear forcibly upon the science of geo[illegible]: and I feel persuaded that even a departure [illegible] theories, if expressed with temperance, and [illegible]easonable arguments, or by undeniable facts, [illegible]he effect of excluding from your pages opinions [illegible]d, must thus become exposed to immediate [illegible]

[illegible] which I propose to make upon the coal [illegible] have [illegible]ention of entering upon the question which, [illegible]pied so much of the attention of geologists, [illegible]ether that valuable substance was of *vege*[illegible] appen [illegible] origin; for I imagine that it will be pretty of [illegible]d that this point has already been completely [illegible] the discoveries of late years, both in geology [illegible]emistry, and that the vegetable origin of coal is now [illegible]ed quite beyond dispute*. Everything, indeed, con[illegible]cted with coal, seems to tend to this important truth. Its immediate and invariable contact with stony strata, in which vegetable substances of every size, from the minutest grasses to the tallest trees, are so beautifully displayed; its bituminous character; its colour; its combustible nature, which is found in no other mineral; its frequently displaying a distinct ligneous texture; and, in corroboration of these, the conclusive experiments and reasoning of many able chemists;—all tend distinctly to prove that the arguments formerly maintained by some geologists, who considered it an original chemical formation, entirely unconnected with a vegetable origin, may now be classed amongst various other concessions which have [illegible] late years, been made to the Wernerian theory [illegible]

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* I have lately had an opportunity of examining a great variety of coal, and have found a distinct vegetable texture in many different specimens; but the most obvious instance I have ever remarked is in the fine coal from Stobart's main near Sunderland, which exhibits in the cleavage, in every part, the appearance of crushed wood, like charcoal. From these and other specimens, I am led to conclude that the soiling quality of most coal is derived from the ligneous portions of it; and that, but for the existence of this substance, the clear and shining bituminous coal would be as clean in the hand as any other specimen of a mineral nature. In all the instances which I have observed, the transverse fracture was clear and brilliant, while between the laminæ the appearance of charcoal was displayed on the surface of each side.—[See our report of Mr. Hutton's paper on the structure and origin of coal, in the preceding volume, p. 302. —Edit.]

Setting out, then, upon the principle th[illegible] situation, *a mineralized vegetable substance*, an[illegible] that it derived its origin from successive de[illegible] of the vegetable productions of former pe[illegible] point of the highest interest to speculate [illegible] which this deposition must have probabl[illegible] upon the evidences which the attendant pha[illegible] hibit, as to the period of time which was occ[illegible] cess.

The first point which arrests our attentio[illegible] tion of the coal-fields of every quarter of [illegible] being *invariably* situated in similar distric[illegible] biting, on every scale of magnitude, the [illegible] I am not aware of any exception to this [illegible] the world in which coal has been discover[illegible] idea, however, of those basin-shaped hollo[illegible]osphere w[illegible] getable deposits have taken place, we must [illegible]e field twic[illegible] attaching to them a great regularity, or rou[illegible] On the contrary, the coal basins are found to be [illegible] in form, as the various lakes and valleys now existing [illegible] present surface of the earth. We find in some places basins of not more than a mile in diameter, and which, in the hills of the West Riding of Yorkshire, are termed *swilleys*, filled like the larger basins with coal and its usual attendant strata. In other districts of greater extent the basin obtains the name of a *field*, extending over many miles of country, but differing in no other material degree from the smaller basins to which I have just alluded. These distinct basin-shaped deposits have greatly tended to support the theory of coal being a *lacustrine* formation, in the supposition that such basins were, at very remote periods, and for a prodigious lapse of years, filled with fresh water; and that the deposition of vegetable substances took place by a slow and very gradual process, and by the submersion of such vegetables as either grew on their shores, or were washed into them by rivers. This mode of accounting for the coal strata, which has long been considered as the most plausible by geologists, is open to insupera-

ble objections, which must occur to the mind of every one who is adequate to the consideration of the present circumstances of inland lakes, even in the most thickly-wooded countries. But without dwelling upon these objections, I shall proceed to notice two facts which have but lately been brought to light, and which appear to me to be of the most conclusive
and utterly destructive of this long-received theory.

of these facts is the very recent discovery of ex-
the coal-fields, containing *sea shells* in great
ch an interesting account has already ap-
from the pen of Mr. J. Phillips, in your
vember 1832. Nor are these marine strata
pect to the beds of coal as to leave the
ir having been actually deposited *by the*
to extend over a very wide district, and it
or a moment supposed, that the sea-shells
accidentally introduced amongst what have
d upon as fresh-water deposits. I have
to me specimens of sea-shells from a bed
Wigan, in Lancashire; and as this locality
stern margin of the same great coal-field
bly traced by Mr. J. Phillips, as having
arine strata, it appears probable that the
y possession have been derived from a
ame beds; although I have not yet ascer-
ar situation in which they were found.
ivalve, of about two inches in diameter, are
pressure, but may still be distinctly defined.
to me to resemble most nearly the genus *Mac-*
nd especially the *M. Lutraria* of Wood's Catalogue.
ngular and conclusive a fact, although similar instances have not yet been brought to light in any great variety of cases, not only affects most materially the very foundation of
he lacustrine theory, but even brings at least one portion of
coal strata distinctly within the catalogue of *marine* de-
and when we consider that the existence of these ma-
ata has only lately become known, not in a new series
nines, but in a district supposed to be perfectly well
to geologists, we cannot but anticipate a speedy in-
to our store of similar facts, as the lights of science begin to shed a more steady lustre on those who are most intimately connected with the coal districts.

The second class of facts to which I before alluded, have even a stronger bearing upon the *manner* of the coal deposits than these marine strata have upon their general character. I mean the existence of large entire trees, in various parts of the strata, *placed in a vertical position*, and intersecting, in many instances, a great variety of beds. It is scarcely necessary to enumerate the instances in which this remarkable position of the fossils has been observed. The facts connected with these vertical stems, *projecting through beds of coal, of shale, and of sandstone*, and assuming in a greater or less d [illegible] gree, the character of each bed with which they come [illegible] tact, must now be well known to all geologists [illegible] appears surprising that the discovery of e [illegible] tree, in any well-defined coal district, sl [illegible] effect of utterly exploding from our [illegible] *slow and gradual deposition*, whether [illegible] Such trees have sometimes been foun [illegible] as it were, in their natural position, [illegible] cases been generally described as i [illegible] ginally *grown* where their stems are [illegible] altogether unnecessary to refute so ex [illegible] tural an idea. For where, in the pres [illegible] we to look for anything analogous o [illegible] argument? Where, in our existing la [illegible] find even the slightest indication of gro [illegible] *intervening strata of sand or clay?* And [illegible] found, where shall we find a tree of 50 [illegible] *in a growing state*, and enduring patient [illegible] *slow lacustrine deposition*, by which its lo [illegible] preserved for the inspection of future g [illegible] which are nourished in the loose sands wh [illegible]

Such considerations are, perhaps, sufficien [illegible] roneous nature of our theories, with respect bo [illegible] and the *relative age* of the coal strata. We must [illegible] either admit the *rapidity* of formation of such deposits as [illegible] hibit entire vertical trees intersecting from 50 to 70 feet o [illegible] variously stratified rock, or we must produce, in the existing system of the world, some instances in the vegetable king [illegible] of *thousands of years' duration*, and so tenacious of lif [illegible] continue growing, and yet not materially increasi [illegible] they become thus gradually covered up *in the bott* [illegible] *water lakes*.

But there is another most important conside [illegible] belongs to this subject, and which militates with equ [illegible] against the theory entertained by some, that deposits of coal have at all times been in progress on the earth, and must even now be going on in the fresh-water lakes of every wooded region. If this had been the case in the very remote epochs assumed by many geologists, we never could have expected to have found the coal strata in the invariable position, with respect to other beds, in which they are actually placed. If, as some able writers have taught, the progress of nature has, in all past ages, been regular and uniform, fresh-water lakes must at all times have existed, vegetable deposits of coal must at all times have b [illegible] gress; and we should consequently have for [illegible] ry part, indiscriminately, of the surface [illegible] wever, is by no means the case; and [illegible] system is more exactly defined than [illegible] cts in which coal may be expected to [illegible]

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* For such a description, see the Annals of Philosophy for November 1820, where an account is given of a tree with roots discovered in the coal sandstone near Glasgow.—[See also a paper by Mr. Witham, in the Phil. Mag. and Annals, N.S., vol. vii. p. 23.—Edit.]

[illegible] superable argument against the theory [illegible] indefined periods of great extent, in [illegible] intersect various parts of the coal-mea- [illegible] action are we to attribute the *rapidity* [illegible] *carbonometers*, if I may so call them, [illegible] Are we to ascribe this rapidity of action [illegible] of the actual strata in which such trees [illegible] are we, on the contrary, to extend the [illegible] gy, to other portions of the series both [illegible] interesting and instructive indexes? We [illegible] ed to reason from analogy, that if any [illegible] nsive series, exhibiting throughout a si- [illegible] can be proved to demonstration to have [illegible] so short a space of time as to cover up and [illegible] *eservation* a vertical stem of 60 or 70 feet in [illegible] er portions of the same series must have been [illegible] a manner extremely similar, if not strictly identi- [illegible] we have, then, a reasonable ground for such conclu- [illegible] can we continue to look upon the coal strata as lacustrine [illegible] ts, from fresh water, formed in the course of *thousands*, [illegible] me think, of *millions* of years? It must be evident that [illegible] facts to which I have alluded are erroneous, or the [illegible] reasoning on the coal-measures must be un- [illegible] facts, however, speak for themselves, and are [illegible] the inspection of every one. It may therefore [illegible] med that suspicion must rest upon the theories [illegible]

In [illegible] per by Mr. J. Phillips to which I have already allude [illegible] we find sections of the coal strata in the neighbourhood of Halifax and Leeds, in one instance amounting to about 50 yards, and in the other to upwards of 170. These

sections, which are of the very same character as those of the mines of the North of England and the South of Scotland, contain no one stratum differing in any material degree from those through which tall and entire stems, of great diameter, have in numerous instances been found to penetrate *vertically*; and it is nearly certain, that if we could bring into one spot the various stems found at various depths in different parts of that coal-field, we should obtain a regular series of *mea-*
sures from the present surface of the ground the ut-
most depths of our coal mines. When we fore,
the evidence adduced by these stems, and th
strata in contact with the coal, we arrive at
clusion, that instead of a long-continued
in the bosom of fresh-water lakes, these
esting formations must have taken place
sea, and by a *violent and rapid process*, at sc
usual destruction in the vegetable kingdom

Although no geological writer has yet, as
brought these *speaking witnesses* to bear
upon the general theory of progressive fo
my own opinion respecting them canno
singular; for Mr. Bakewell, in his ver
to Geology," makes the following rema
treating of the coal-measures. "In so
tions have been made in the sandstone
coal, instances of fossil stems of large pla
tical position. In Burntwood quarry, at Alt
field in Yorkshire" (which is situated in the ve
as Halifax and Leeds, where the *marine* stra
marked by Mr. J. Phillips,) "*several vertical*
magnitude have been found. One stem which I m
the quarry, was 9 feet long and 10 inches in thickness
what is very remarkable, this stem cut through three str
sandstone *parted by regular strata seams.* It had there
observes this author, (not being aware, at the time he
of the intermixture of *marine strata* amongst these very
tions,)—"it had therefore probably *grown where i*
it is difficult to believe that any vegetable stem
through three strata of sandstone, the lower of wh
must have been partly *consolidated. This fact p*
strata were deposited rapidly." p. 148. To the y"
here mentioned by Mr. Bakewell, I shall only a still
greater one of believing that a living tree could be rapidly covered up *where it grew*, and its top imbedded in the very same substance in which its roots had before found nourish-

ment. Had this strange idea been well founded, we must have discovered such fossil trees in many more instances than we do: they must have been almost always upright, and invariably furnished with *roots* and *branches*. This, however, is rarely the case; they lie in every degree of inclination from horizontal to vertical: they have not always roots, very rarely branches, and I have never heard of an instance with the smaller twigs and leaves. If this able geologist was deeply struck with a fossil stem of only *nine* feet, and piercing *three* strat [illegible] andstone, what must he think of such stems as have
si [illegible] equently found (as in Craigleith quarry near Edin-
[illegible] m 50 to 70 feet in length, *and piercing ten or*
[illegible] *distinct strata* * ?

[illegible] ng these cursory remarks on the coal for-
[illegible] lso desirous of calling the attention of your
[illegible] s to another very remarkable fact, with regard
[illegible] iferous strata, which, I believe, has only been
[illegible] e same able writer whom I have just cited, and
[illegible] s to me to offer a powerful resistance to our
[illegible] ries respecting them.

[illegible] the remarkable deposits of coal near Co-
[illegible] e imbedded “ trunks of trees deprived of
[illegible] *ich proves their having been transported*
[illegible] d also nuts which are indigenous to *Hin-*
[illegible] Mr. Bakewell proceeds as follows: “ But
[illegible] kable formation occurs at Alpnach near the
[illegible] in Switzerland, where a bed of coal is found
[illegible] of 280 feet from the surface. Over this coal there
[illegible] *stratum of limestone containing fluviatile*” (?) “*shells, and*
[illegible] *e bones and teeth of a species of Mastodon, and of other large land quadrupeds.* Notwithstanding the occurrence of the bones of large mammalia in this stratum over the coal, in this place, the coal approaches in character nearly to mineral coal; and *the strata of micaceous sandstones and shales above it, have a close resemblance to those of our English coal-fields*: but though from these organic remains we are *compelled* to place the coal of Alpnach amongst the *tertiary strata*, or else to admit the occurrence of an anomalous formation like the one at Stonesfield, still I believe the true geological position of this coal is problematical; and it deserves the particular attention of some

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* [A notice of the fossil tree discovered at Craigleith in 1826 will be found in Mr. Witham's paper before referred to; but this tree, we have to remark, was found in a nearly horizontal position, corresponding with the dip of the strata.—EDIT.]

English geologist well acquainted with the coal-fields of his own country, and the lignite formation in different parts of Europe." p. 173.

The singular anomaly here presented to the geologist powerfully supports the argument which naturally arises from the specimens of vertical trees, to which allusion has just been made. According to geological theories, such deposits of vegetable matter, *accompanied by micaceous sandstones and shales*. were formed *long previous* to the creation of such mammalia as are here described*; and yet we have only to
e the
highly probable existence of vertical trees in this
of
Alpnach, such as are elsewhere found in simil
perceive the rapidity of this formation also,
place at a period when the destruction of anim
as our upper diluvial deposits more usually dem

I must apologize for having thus trespassed
tention and that of your readers; but I cann
that the facts I have mentioned are well worth
study by all true lovers of a consistent geol
line of reasoning upon these facts may, in the
be open to many objections; but, be that as
be admitted that a scientific explanation of the
opposed as they are to the most approved
would be considered as an essential servic
interesting science, and that they loudly
interference of some of the great leaders
world. I have the honour to be,

Gentlemen, yours, &c.

Ramsgate, June 17, 1833. GEO. FAIRHOLME.

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* [We leave Mr. Fairholme's theory of the rapid production of the coal strata to the consideration of our geological readers and correspondents; but it seems requisite that we should offer a remark on his allusion to the coal of Alpnach. It is not "according to geological theories" (by which we mean the generalizations of facts which have been established by modern geologists,) to suppose that such strata as those of Alpnach here alluded to, were formed "*long previous*" to the creation of the mammalia in question: all that can be deduced from the occurrence of remains of the latter in the stratum overlying the coal, is that the entire formation belongs to the tertiary series, and probably the animals were contemporaneous with the plants which furnished the coal. It is now fully established that coal has been produced at many epochs subsequent to that of the Great Coal Formation; and we may also observe, that no geologist of the present day would infer the high antiquity of a formation containing coal, merely from the mineral characters of the accompanying strata.—EDIT.]

XLV. *Experimental Researches in Electricity.—Third Series.* *By* Michael Faraday, *D.C.L. F.R.S. M.R.I. Fullerian Prof. Chem. Royal Institution, Corr. Mem. Royal Acad. of Sciences, Paris, Petersburgh, &c. &c.*

[Continued from p. 171.]

309. iii. *Chemical Decomposition.* THE chemical action of voltaic electricity is characteristic of that agent, but not more characteristic than are the laws under which the bodies evolved by decomposition arrange themselves at the poles. Dr. Wollaston showed* that common electricity resembled it in these effects, and "that they are both essentially the same;" but he mingled with his proofs an experiment having a resemblance, and nothing more, to a case of voltaic decomposition, which however he himself partly distinguished; and this has been more frequently referred to by others, on the one hand, to prove the occurrence of electro-chemical decomposition, like that of the pile, and on the other, to throw doubt upon the whole paper, than the more numerous and decisive experiments which he has detailed.

310. I take the liberty of describing briefly my results, and of thus adding my testimony to that of Dr. Wollaston on the identity of voltaic and common electricity as to chemical action, not only that I may facilitate the repetition of the experiments, but also lead to some new consequences respecting chemical decomposition (376. 377.).

311. I first repeated Wollaston's fourth experiment†, in which the ends of coated silver wires are immersed in a drop of sulphate of copper. By passing the electricity of the machine through such an arrangement, that end in the drop which received the electricity became coated with metallic copper. One hundred turns of the machine produced an evident effect; two hundred turns a very sensible one. The decomposing action was however very feeble. Very little copper was precipitated, and no sensible trace of silver from the other pole appeared in the solution.

312. A much more convenient and effectual arrangement for chemical decompositions by common electricity, is the following. Upon a glass plate, fig. 2, placed over, but raised above a piece of white paper, so that shadows may not interfere, put two pieces of tinfoil *a*, *b*; connect one of these by an insulated wire *c*, or wire and string (301.) with the machine, and

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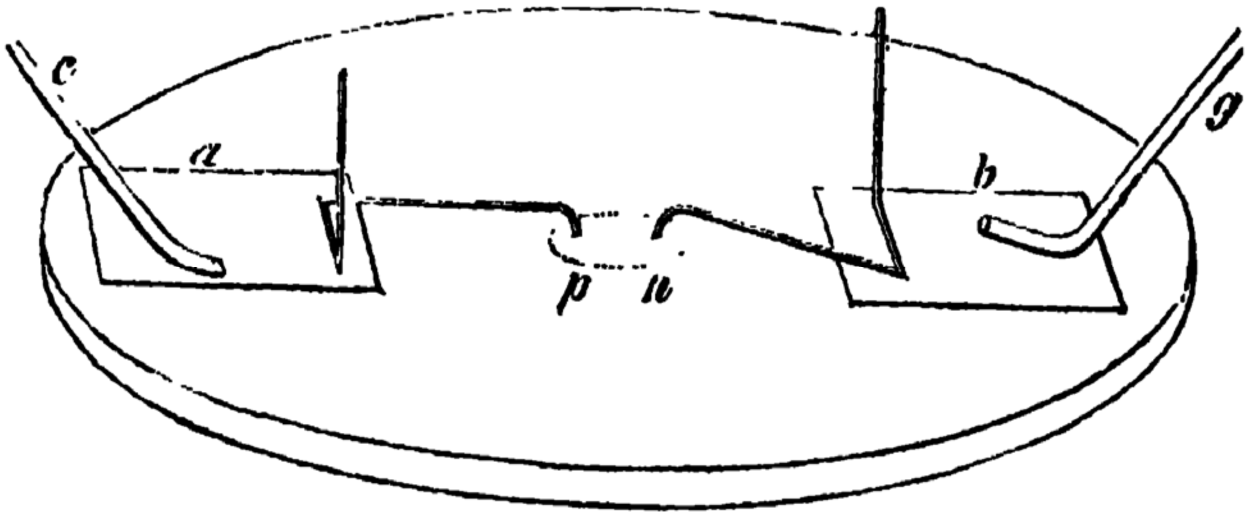
* Phil. Trans. 1801, pp. 427, 434.

† Ibid. 1801, p. 429.

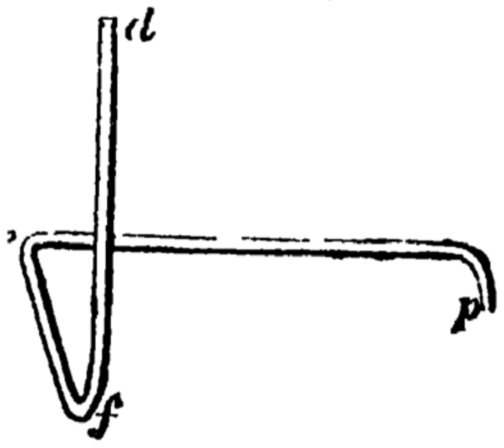
the other *g*, with the discharging train (292.) or the negative

Fig. 2.



conductor; provide two pieces of fine platina wire, bent as in fig. 3, so that the part *d*, *f* shall be nearly upright, whilst the whole is resting on the three bearing points *p*, *e*, *f*; place these as in fig. 2; the points *p*, *n* then become the decomposing poles. In this way surfaces of contact, as minute as possible, can be obtained at pleasure, and the connexion can be broken or renewed in a moment, and the substances acted upon examined with the utmost facility.

Fig. 3.



313. A coarse line was made on the glass with solution of sulphate of copper, and the terminations *p* and *n* put into it; the foil *a* was connected with the positive conductor of t machine by wire and wet string, so that no sparks pas twenty turns of the machine caused the precipita[illegible] much copper on the end *p*, that it looked like copper wire; no apparent change took place at *n*.

314. A mixture of half muriatic acid and half water was rendered deep blue by sulphate of indigo, and a large drop put on the glass, fig. 2, so that *p* and *n* were immersed at opposite sides: a single turn of the machine showed bleaching effects round *p*, from evolved chlorine. After twenty revolutions no effect of the kind was visible at *n*, but so much chlorine had been set free at *p*, that when the drop was stirred the whole became colourless.

315. A drop of solution of iodide of potassium mingled with starch was put into the same position at *p* and *n*; on turning the machine, iodine was evolved at *p*, but not at *n*.

316. A still further improvement in this form of apparatus consists in wetting a piece of filtering paper in the solution to be experimented on, and placing that under the points *p* and *n*, on the glass: the paper retains the substance evolved at the point of evolution, by its whiteness renders any change of

colour visible, and allows of the point of contact between it and the decomposing wires being contracted to the utmost degree. A piece of paper moistened in the solution of iodide of potassium and starch, or of the iodide alone, with certain precautions (322.), is a most admirable test of electro-chemical action; and when thus placed and acted upon by the electric current, will show iodine evolved at *p* by only half a turn of the machine. With these adjustments and the use of iodide of potassium on paper, chemical action is sometimes a more delicate test of electrical currents than the galvanometer (273.). Such cases occur when the bodies traversed by the current are bad conductors, or when the quantity of electricity evolved or transmitted in a given time is very small.

317. A piece of litmus paper, moistened in solution of common salt or sulphate of soda was quickly reddened at *p*. A similar piece moistened in muriatic acid was very soon bleached at *p*. No effects of a similar kind took place at *n*.

318. A piece of turmeric paper, moistened in solution of sulphate of soda, was reddened at *n* by two or three turns of the machine, and in twenty or thirty turns plenty of alkali was there evolved. On turning the paper round, so that the spot came under *p*, and then working the machine, the alkali soon disappeared, the place became yellow, and a brown alkaline spot appeared in the new part under *n*.

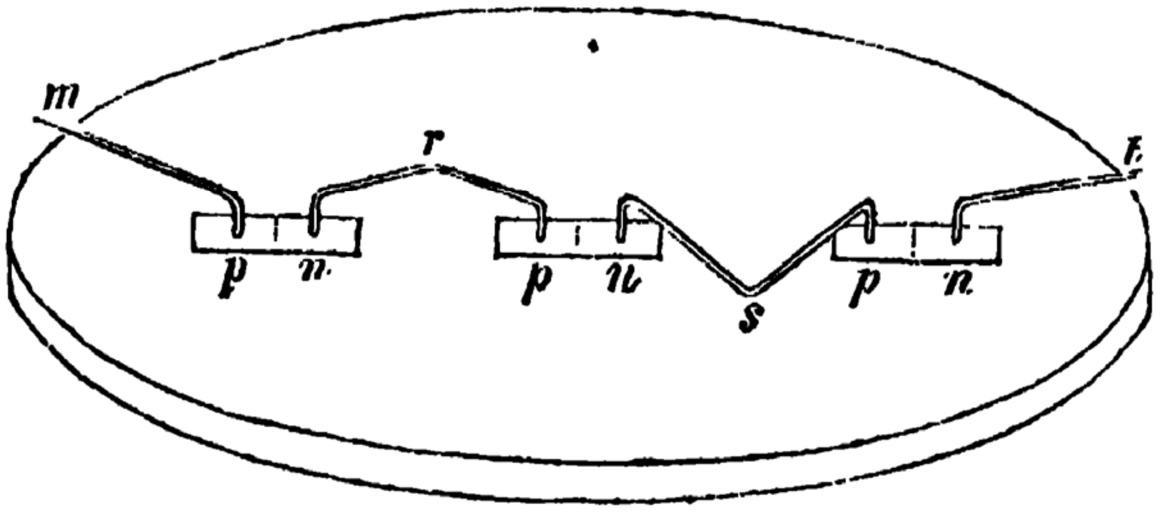
319. On combining a piece of litmus with a piece of turmeric paper, wetting both with solution of sulphate of soda, and putting the paper on the glass, so that *p* was on the litmus and *n* on the turmeric, a very few turns of the machine sufficed to show the evolution of acid at the former and alkali at the latter, exactly in the manner effected by a volta-electric current.

320. All these decompositions took place equally well, whether the electricity passed from the machine to the foil *a*, through water, or through wire only; by *contact* with the conductor, or by *sparks* there; provided the sparks were not so large as to cause the electricity to pass in sparks from *p* to *n*, or towards *n*; and I have seen no reason to believe that in cases of true electro-chemical decomposition by the machine, the electricity passed in sparks from the conductor, or at any part of the current, is able to do more, because of its tension, than that which is made to pass merely as a regular current.

321. Finally, the experiment was extended into the following form, supplying in this case the fullest analogy between common and voltaic electricity. Three compound pieces of litmus and turmeric paper (319.) were moistened in solution of sulphate of soda, and arranged on a plate of glass with

platina wires, as in fig. 4. The wire *m* was connected with the prime conductor of the machine, the wire *t* with the dis-

Fig. 4.



charging train, and the wires *r* and *s* entered into the course of the electrical current by means of the pieces of moistened paper: they were so bent as to rest each on three points, *n*, *r*, *p*; *n*, *s*, *p*, the points *r* and *s* being supported by the glass, and the others by the papers, the three terminations *p*, *p*, *p* rested on the litmus, and the other three *n*, *n*, *n* on the turmeric paper. On working the machine for a short time only, acid was evolved at *all* the poles or terminations *p*, *p*, *p*, by which the electricity entered the solution, and alkali at the other poles *n*, *n*, *n*, by which the electricity left the solution.

322. In all experiments of electro-chemical decomposition by the common machine and moistened papers (316.), it is necessary to be aware of and to avoid the following important source of error. If a spark passes over moistened litmus and turmeric paper, the litmus paper (provided it be deli[illegible]nd not too alkaline,) is reddened by it; and if several sparks are passed, it becomes powerfully reddened. If the electricity pass a little way from the wire over the surface of the moistened paper, before it finds mass and moisture enough to conduct it, then the reddening extends as far as the ramifications. If similar ramifications occur at the termination *n*, on the turmeric paper, they *prevent* the occurrence of the red spot due to the alkali, which would otherwise collect there; sparks or ramifications from the points *n* will also redden litmus paper. If paper moistened by a solution of iodide of potassium (which is an admirably delicate test of electro-chemical action,) be exposed to the sparks or ramifications, or even a feeble stream of electricity through the air from either the point *p* or *n*, iodine will be immediately evolved.

323. These effects must not be confounded with those due to the true electro-chemical powers of common electricity, and must be carefully avoided when the latter are to be observed. No sparks should be passed, therefore, in any part

of the current, nor any increase of intensity allowed by which the electricity may be induced to pass between the platina wires and the moistened papers, otherwise than by conduction; for if it burst through the air, the effect referred to ensues.

324. The effect itself is due to the formation of nitric acid by the combination of the oxygen and nitrogen of the air, and is, in fact, only a delicate repetition of Cavendish's beautiful experiment. The acid so formed, though small in quantity, is in a high state of concentration as to water, and produces the consequent effects of reddening the litmus paper, or preventing the exhibition of alkali on the turmeric paper, or, by acting on the iodide of potassium, evolving iodine.

325. By moistening a very small slip of litmus paper in solution of caustic potassa, and then passing the electric spark over its length in the air, I gradually neutralized the alkali, and ultimately rendered the paper red; on drying it, I found that nitrate of potassa had resulted from the operation, and that the paper had become touch-paper.

326. Either litmus paper or white paper moistened in solution of iodide of potassium, offers therefore a very simple, beautiful, and ready means of illustrating Cavendish's experiment of the formation of nitric acid from the atmosphere.

327. I have already had occasion to refer to an experiment (265. 309.) by Dr. Wollaston, which is insisted upon too much, both by those who oppose and those who agree with the accuracy of his views respecting the identity of voltaic and ordinary electricity. By covering fine wires with glass or other insulating substances, and then removing only so much matter as to expose the point, or a section of the wires, and by passing electricity through two such wires, the guarded points of which were immersed in water, Wollaston found that the water could be decomposed even by the current from the machine, without sparks, and that two streams of gas arose from the points, exactly resembling in appearance, those produced by voltaic electricity, and, like the latter, giving a mixture of oxygen and hydrogen gases. But Dr. Wollaston himself points out that the effect is different from that of the voltaic pile, inasmuch as both oxygen and hydrogen are evolved from *each* pole; he calls it "a very close *imitation* of the galvanic phænomena," but adds, that "in fact the resemblance is not complete," and does not trust to it to establish the principles, correctly laid down in his paper.

328. This experiment is neither more nor less than a repetition, in a refined manner, of that made by Dr. Pearson in

1797*, and previously by MM. Paets Van Troostwyk and Deiman in 1789 or earlier. That the experiment should never be quoted as proving true electro-chemical decomposition, is sufficiently evident from the circumstance, that the law which regulates the transference and final place of the evolved bodies (278. 309.) has no influence here. The water is decomposed at both poles independently of each other, and the oxygen and hydrogen evolved at the wires are the elements of the water existing the instant before in those places. That the poles, or rather points, have no mutual decomposing dependence, may be shown by substituting a wire, or the finger, for one of them, a change which does not at all interfere with the other, though it stops all action at the changed pole. This fact may be observed by turning the machine for some time; for though bubbles will rise from the point left unaltered, in quantity sufficient to cover entirely the wire used for the other communication, if they could be applied to it, yet not a single bubble will appear on that wire.

329. When electro-chemical decomposition takes place, there is great reason to believe that the quantity of matter decomposed is not proportionate to the intensity, but to the quantity of electricity passed (320.). Of this I shall be able to offer some proofs in a future part of this paper (375. 377.). But in the experiment under consideration, this is not the case. If, with a constant pair of points, the electricity be passed from the machine in sparks, a certain proportion of gas is evolved; but if the sparks be rendered shorter, less gas is evolved, and if no sparks be passed, there is scarcely a sensible portion of gases set free. On substituting solution of sulphate of soda for water, scarcely a sensible quantity of gas could be procured even with powerful sparks, and almost none with the mere current; yet the quantity of electricity in a given time was the same in all these cases.

330. I do not intend to deny that with such an apparatus common electricity can decompose water in a manner analogous to that of the voltaic pile; I believe at present that it can. But when what I consider the true effect only was obtained, the quantity of gas given off was so small that I could not ascertain whether it was, as it ought to be, oxygen at one wire and hydrogen at the other. Of the two streams one seemed more copious than the other; and on turning the apparatus round, still the same side in relation to the machine gave the largest stream. On substituting solution of sulphate of soda for pure water (329.), these minute streams were still

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* Nicholson's Journal, 4to, vol. i. pp. 241, 299, 349.

observed. But the quantities were so small, that on working the machine for half an hour I could not obtain at either pole a bubble of gas larger than a small grain of sand. If the conclusion which I have drawn (377.) relating to the amount of chemical action be correct, this ought to be the case.

331. I have been the more anxious to assign the true value of this experiment as a test of electro-chemical action, because I shall have occasion to refer to it in cases of supposed chemical action by magneto-electric and other electric currents (336. 346.) and elsewhere. But, independent of it, there cannot be now a doubt that Dr. Wollaston was right in his general conclusion; and that voltaic and common electricity have powers of chemical decomposition, alike in their nature, and governed by the same law of arrangement.

332. iv. *Physiological Effects.*—The power of the common electric current to shock and convulse the animal system, and when weak to affect the tongue and the eyes, may be considered as the same with the similar power of voltaic electricity, account being taken of the intensity of the one electricity and duration of the other. When a wet thread was interposed in the course of the current of common electricity from the battery (291.) charged by eight or ten revolutions of the machine in good action (290.), and the discharge made by platina spatulas through the tongue or the gums, the effect upon the tongue and eyes was exactly that of a feeble voltaic circuit.

333. v. *Spark.*—The beautiful flash of light attending the discharge of common electricity is well known. It rivals in brilliancy, if it does not even very much surpass, the light from the discharge of voltaic electricity; but it endures for an instant only, and is attended by a sharp noise like that of a small explosion. Still no difficulty can arise in recognising it to be the same spark as that from the voltaic battery, especially under certain circumstances. The eye cannot distinguish the difference between a voltaic and a common electricity spark, if they be taken between amalgamated surfaces of metal, at intervals only, and through the same distance of air.

334. When the battery (291.) was discharged through a wet string placed in some part of the circuit away from the place where the spark was to pass, the spark was yellowish, flamy, having a duration sensibly longer than if the water had not been interposed, was about three fourths of an inch in length, was accompanied by little or no noise, and whilst losing part of its usual character had approximated in some degree to the voltaic spark. When the electricity retarded by water was discharged between pieces of charcoal, it was exceedingly luminous and bright upon both surfaces of the

charcoal, resembling the brightness of the voltaic discharge on such surfaces. When the discharge of the unretarded electricity was taken upon charcoal, it was bright upon both the surfaces, (in that respect resembling the voltaic spark,) but the noise was loud, sharp and ringing.

335. I have assumed, in accordance, I believe, with the opinion of every other philosopher, that atmospheric electricity is of the same nature with ordinary electricity (284.), and I might therefore refer to certain published statements of chemical effects produced by the former as proofs that the latter enjoys the power of decomposition in common with voltaic electricity. But the comparison I am drawing is far too rigorous to allow me to use these statements without being fully assured of their accuracy; and I have no right to suppress them, because, if accurate, they establish what I am labouring to put on an undoubted foundation, and have priority to my results.

336. M. Bonijol of Geneva* is said to have constructed very delicate apparatus for the decomposition of water by common electricity. By connecting an insulated lightning rod with this apparatus, the decomposition of the water proceeded in a continuous and rapid manner even when the electricity of the atmosphere was not very powerful. The apparatus is not described; but as the diameter of the wire is mentioned as very small, it appears to have been similar in construction to that of Wollaston (327.); and as that does not furnish a case of true polar electro-chemical decomposition (328.), this result of M. Bonijol does not prove the identity in chemical action of common and voltaic electricity.

337. At the same page of the *Bibliotheque Universelle*, M. Bonijol is said to have decomposed *potash*, and also chloride of silver, by putting them into very narrow tubes and passing electric sparks from an ordinary machine over them. It is evident that these offer no analogy to cases of true voltaic decomposition, where the electricity only decomposes when it is *conducted* by the body acted upon, and ceases to decompose, according to its ordinary laws, when it passes in sparks. These effects are probably partly analogous to that which takes place with water in Pearson's or Wollaston's apparatus, and may be due to very high temperature acting on minute portions of matter; or they may be connected with the results in air (322.). As nitrogen can combine directly with oxygen under the influence of the electric spark (324.), it is not impossible that it should even take it from the potassium of the potash,

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* *Bibliotheque Universelle*, 1830, tome xlv. p. 213.

especially as there would be plenty of potassa in contact with the acting particles to combine with the nitric acid formed. However distinct all these actions may be from true polar electro-chemical decompositions, they are still highly important, and well worthy of investigation.

338. The late Mr. Barry communicated a paper to the Royal Society* last year, so distinct in the details that it would seem at once to prove the identity in chemical action of common and voltaic electricity, but that, when examined, considerable difficulty arises in reconciling certain of the effects with the remainder. He used two tubes, each having a wire within it passing through the closed end, as is usual for voltaic decompositions. The tubes were filled with solution of sulphate of soda, coloured with syrup of violets, and connected by a portion of the same solution, in the ordinary manner; the wire in one tube was connected by a *gilt thread* with the string of an insulated electrical kite, and the wire in the other tube by a similar *gilt thread* with the ground. Hydrogen soon appeared in the tube connected with the kite, and oxygen in the other, and in ten minutes the liquid in the first tube was green from the alkali evolved, and that in the other red from free acid produced. The only indication of the strength of the atmospheric electricity is in the expression, " the usual shocks were felt on touching the string."

339. That the electricity in this case does not resemble that from any ordinary source of common electricity, is shown by several circumstances. Wollaston could not effect the decomposition of water by such an arrangement, and obtain the gases in *separate* vessels, using common electricity; nor have any of the numerous philosophers, who have employed such an apparatus, obtained any such decomposition, either of water or of a neutral salt, by the use of the machine. I have lately tried the large machine (290.) in full action for a quarter of an hour, during which seven hundred revolutions were made without producing any sensible effects, although the shocks that it would then give must have been far more powerful and numerous than could have been taken, with any chance of safety, from an electrical kite-string; and by reference to the comparison hereafter to be made (371.), it will be seen that for common electricity to have produced the effect, the quantity must have been awfully great, and apparently far more than could have been conducted to the earth by a gilt thread, and at the same time only have produced the " usual shocks."

340. That the electricity was apparently not analogous to

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* Philosophical Transactions, 1831, p. 165.

voltaic electricity, is evident, for the "usual shocks" only were produced, and nothing like the terrible sensation due to a voltaic battery, even when it has a tension so feeble as not to strike through the eighth of an inch of air.

341. It seems just possible that the air which was passing by the kite and string, being in an electrical state sufficient to produce the "usual shocks" only, could still, when the electricity was drawn off below, renew the charge, and so continue the current. The string was 1500 feet long, and contained two double threads. But when the enormous quantity which must have been thus collected is considered (371. 376.), the explanation seems very doubtful. I charged a voltaic battery of twenty pairs of plates four inches square and with double coppers, very strongly, insulated it, connected its positive extremity with the discharging train (292.), and its negative pole with an apparatus like that of Mr. Barry, communicating by a wire inserted three inches into the wet soil of the ground. This battery thus arranged produced feeble decomposing effects, as nearly as I could judge answering the description M. Barry has given. Its intensity was, of course, far lower than the electricity of the kite-string, but the supply of quantity from the discharging train was unlimited. It of course gave no shocks to compare with the "usual shocks" of a kite-string.

342. Mr. Barry's experiment is a very important one to repeat and verify. If confirmed, it will be, as far as I am aware, the first recorded case of true electro-chemical decomposition of water by common electricity, and it will supply a form of electrical current, which, both in quantity and intensity, is exactly intermediate between those of the common electrical machine and the voltaic pile.

[To be continued.]

XLVI. *Experimental Contributions towards the Theory of Thermo-electricity. By Mr.* JOHN PRIDEAUX, *Member of the Plymouth Institution.*

[Concluded from p. 215.]

V. *What are the electrical conditions of Homogeneous Metals brought into contact at different temperatures?*

19. TWO copper wires, $\frac{1}{16}$th of an inch in diameter, had a flat spiral, $\frac{3}{4}$ in diameter, turned in each, the central end of the wire being drawn out perpendicular to the spiral, to enter a cork, for a handle; the external end being continued in a line, nearly parallel with the face of the spiral, for 8

inches, and the point amalgamated. Thus the faces of the spirals could be pressed against each other, by means of the cork handles, the amalgamated ends entering the mercury boxes to communicate with the magnetcst. One of the spirals being now heated, and (having placed the points in the mercury boxes,) pressed against the cold one, a deflection took place of 5°, showing an electric current from the hot spiral to the cold one. The heat employed was about 300°.

20. An iron pair, formed in the same way, being similarly employed, gave a deflection in the opposite direction, indicating the current *from* the *cold to* the *hot* spiral, which increased by increased heat till, on approaching redness, it reached 18°.

A pair, of tinned iron, being next employed gave the same result, but in a much greater degree. The mere warmth of the fingers communicated to one of the spirals produced deflections of 3° or 4°; and before the heat reached redness, it was 30°; the electric current being in this instance also from cold to hot.

21. Platinum, silver, lead, tin, and bismuth all corresponded with copper, in the currents passing from the hot to the cold metal. Bismuth could not, of course, be obtained in spirals; and therefore rods of the other metals were employed to compare with it. This property, in which the cold metal is positive to the hot (in the common thermo-electric sense*), may be called thermo-negative. Bismuth possesses it in the highest degree; then silver; next platinum, copper, tin. Lead, whether in wire or in bars, has only just enough of it to recognise.

22. Zinc and antimony correspond with iron, or are thermo-positive; that is, the hot metal is positive to the cold. But the experimenter will have some trouble, at first, in ascertaining the point.

Rolled zinc gives a current from hot to cold, when heated up to about 200°; between that and 250° is a neutral point; and from 400° up to the melting point the current is more lively, but reversed; *i. e.* from cold to hot, or thermo-positive. I have hitherto had some difficulty as to the temperature at which the change takes place. When heated up to the thermo-positive point, it seems to retain that character in cooling, down to a temperature considerably below that at which it first assumes it; and the direction of the heat, in the heated bar, seems to exercise an influence in the effect. This property of zinc explains the discrepancy respecting it in the tables of Becquerel and Cumming; the former having worked below 200°, the latter much above that point: and in this thermo-electric change of the same metal, at such manageable temperatures, the secret seems to lie within narrow compass: it promises to contain the key by which the whole field may be laid open. Zinc bars were more troublesome, some being always negative to others, whichever is heated. A long bar, well cast, broken in the middle, and the contiguous ends used for the points of contact, generally act consistently. But even here the surfaces of contact must be filed, not hammered flat; for a few blows of the hammer destroy the consistency of action. The crystalline texture is pretty evidently concerned in this inconstancy.

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* It must be noticed that the terms 'positive' and 'negative' have been fixed in thermo-electricity in the sense of electrics, not of conductors; positive here being that which takes, not that which gives, positive electricity.

Two slips of rolled zinc, cut side by side, from the same sheet, and the heads doubled, or trebled, to retain the heat, have always given me consistent results, so far as I have been able to understand them.

But it should be observed, that in this and other metals (except lead) cast rods are more active than rolled or wire-drawn pieces.

23. Antimony is still more embarrassing than zinc. It cannot be rolled or drawn, and is difficult to cast well. A tolerably well cast bar, broken in the middle, and used as described with zinc, was consistent as to direction of the current, but far otherwise as to its force, when the pieces were consecutively heated to the same degree, and applied one hot to the other cold. When heated together, one half was always positive to the other, as though the other had been of a less active metal. The method which I found to answer best, was, to take as many well cast bars as I could obtain, and classify them, by heating them together in pairs; putting the negative bars on one side, and the positive on the other; then, proceeding in the same manner, to separate the negative bars from each other; and so on, until a pair was obtained which gave no deflection when heated together. Such bars gave consistent results when heated separately, the current being always thermo-positive, in about the same degree as that of iron.

It is singular that bismuth, a very crystallizable metal, has never given me any embarrassment when acting in this way, although sufficiently capricious in single masses (26.).

24. Thus the metals resolve themselves into two classes:

1. Thermo-positive*; those which become increasingly positive by heating.—Antimony, iron, zinc above 250°.

2. Thermo-negative*; those which heat renders increas-

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* These terms are not unobjectionable; but 'idio-positive' and 'idio-negative' would seem to apply rather to the cold than the hot bar; and 'thermo-electro-positive' is too complex for an English word.

ingly negative.—Silver, platinum, copper, tin, lead; and zinc below 200°.

Of the first class, the thermo-electric order (acting in pairs of different metals,) is nearly the reverse of that of conduction.

Of the second class, the thermo-electric order nearly corresponds with that of conduction.

Tables of conduction from Mr. Forbes.		Thermo-electric table from Prof. Cumming.
Heat.	Electricity.	
Gold.	Silver.	Antimony.
Silver.	Copper.	Iron.
Copper.	Gold.	Zinc*.
Iron.	Zinc.	Silver.
Zinc.	Iron.	Copper.
Platinum.	Platinum.	Gold.
Tin.	Tin.	Lead.
Lead.	Lead.	Tin.
Antimony.	Antimony	Platinum.
Bismuth.	Bismuth.	Bismuth.

Mr. Forbes considers the electrical table better authenticated than that for heat, and seems inclined to the opinion, that the order is really identical for the two.

Thermo-positive class, in their thermo-positive order.	Order of conduction.	Order of thermo-electric reaction.
Antimony.	Zinc.	Antimony.
Iron†.	Iron.	Iron.
Zinc, above 250°.	Antimony.	Zinc.
Thermo-negative class, in their thermo-negative order.		
Bismuth.	Silver.	Silver.
Silver.	Copper.	Copper.
Platinum.	Zinc.	Lead.
Copper.	Platinum.	Tin.
Tin.	Tin.	Platinum.
Zinc, below 200°.	Lead.	Bismuth.
Lead.	Bismuth.	

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* Becquerel brings zinc below silver, for reasons already adverted to (22.).

† Iron is well known to have its thermo-electric reaction reversed at a bright red heat, and to undergo the same change in its magnetic relations,—a remarkable analogy between the two. This would have been noticed above, but from the difficulty of the investigation having rendered my results hitherto inconstant. When the red-hot part is thin, its surface of contact loses its redness on touching the cold face; when substantial enough to prevent this, it is not easy to heat it high enough, without oxidation, and without the communication of some warmth to the points in contact with the mercury.

25. On comparing these tables, the thermo-electric reaction, in pairs of different metals, would seem to be compounded of the idio-thermo-electric force of each, and its conducting power.

In the thermo-positive class, the order of conduction is reversed: here iron and antimony differ little in thermo-positive quality; but antimony is the worst conductor, and stands highest in Cumming's table.

Zinc and iron differ little in conducting power; but iron is considerably more thermo-electric than zinc, and accordingly predominates in action when the metals are acting together.

In the thermo-negative class, the arrangement corresponds with the order of conduction; the conducting power tending to raise, the thermo-negative to lower, the metal in Cumming's table.

Silver is a better conductor than copper, but at the same time more thermo-negative, and their thermo-electric reaction is slight and uncertain; copper, though usually described as negative to silver, being sometimes positive, particularly at high temperatures: I have, indeed, most frequently found it so.

Copper is a better conductor than platinum, and much less thermo-negative; the one property tends to keep up the copper, the other to lower the platinum, which accordingly stands much below copper in Cumming's table.

Platinum is a little better conductor than tin, but much more thermo-negative: the latter quality accordingly predominates, and tin rises above platinum.

Lead, a worse conductor than either of these metals, is very much behind them in thermo-negative property, and both accordingly fall below it in the table of mutual reaction.

Bismuth, the worst conductor, and the strongest thermo-negative metal, has both properties coinciding to place it at the bottom of the table, and would, if the forces were measured, stand as far below platinum as that metal does (the temperature being alike,) below silver.

The place of zinc, in this class, cannot be so well settled, as it changes with the temperature.

To reduce these proportions to numbers would require more precise knowledge of the conducting powers than we yet possess, and a greater variety of experiments on the idio-thermo-electric powers than an individual is likely to make. The proximate cause of this peculiar faculty comes first in the order of inquiry.

VI. *Can the currents which pervade a mass of bismuth (and other metal), when heated at a point, be drawn off by conductors, so as to be traced up and connected with its thermonegative property?*

26. Mr. Sturgeon found that a solid prism of bismuth, heated at a point near the end, gave out an electric stream, which flowed along the side where the heated point lay, ran over the edges before it reached the other end, and returned along the opposite side to the point of heat*.

I cast several slabs, prisms, and cylinders of this metal of various dimensions, some being cast flat, others on end, but could not obtain the regularity of circuit described by Sturgeon. In every case in which a current was detected from the heated point, another was found returning towards it; but it did not always happen that this was on the opposite side. Sometimes, on a slab 4 inches wide, it was on the lateral parts of the same face, more often on the edges: in several instances the current was towards the heated point, along the middle of the same face, and back again along the lateral parts or edges. In many cases the current took the same direction on the opposite sides, and returned along the edges. One slab, cast just at the melting point, in a cold mould, crystallized in granules; and in this no currents were detected.

Each piece was tried with a point of heat about the middle of each end of every face, allowing it to cool perfectly between the successive heatings. The heat was applied by the dart of the spirit blow-pipe, and at various degrees, a melted spot being produced in a few seconds. The bismuth was instantly raised close under the lower of a pair of short needles, not quite astatic, the needles being 2 inches apart. They were suspended by a fibre of unspun silk, within a glass shade, closed at the bottom with writing-paper, on which was a divided circle and a guide-cross as described (1.) p. 207, only the cross was here drawn on the paper, as the thickness of card between the bismuth and the needle would have been too great in some cases.

27. A cylindrical bar (10 × $\frac{5}{8}$ inches) was filed up to a bright surface, and two copper wires made to slide upon it, the ends being pressed against it by a couple of card hoops, which slid with the wires. The other ends of the wires were amalgamated, and dipped into the mercury boxes of the magnetest. The bar being now heated at a given point, the direction of

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* Phil. Mag. and Annals, N.S. vol. x. p. 15. I attribute this to Mr. Sturgeon, not having seen the description of Chevalier Yelin's experiments, and therefore being unable to say whether he anticipated this one or not.

the currents was traced by the needles last described (26.). The ends of the copper wires were now brought into the line of the current, (by turning and sliding the hoops,) in the hope that, by the superior conducting power of the copper, either the direct or return current might be taken up by the wire. But I could not succeed in obtaining this, or the vestige of any current, through the wire, until, coming in contact with a warm part of the bismuth, it assumed its proper thermo-electric action.

28. Two square bars, each connected at foot with the magnetest, and the head of one heated, were brought into contact at the upper ends: the current *always* set from the hot into the cold bar (21.), though in the majority of cases it would have gone down the heated face of the hot bar, and up the cold face.

29. On heating one of these bars at midlength, so that the heat should not readily reach either end, the current was always from the cold to the heated bar, although in that single bar (*i. e.* in either singly,) it would have been in different directions, according to the side heated.

30. Two bismuth rods, each 8 × $\frac{1}{4}$ inches, were tied together at top, the feet communicating with the magnetest: on directing the blow-pipe dart to the midlength of either, a current immediately set from the cold rod to the one so heated, as in (29.); but as the heat extended itself through the rod, the needle gradually returned to its station, and passed to the other side, as usual with two bars of that metal, one hot, the other cold, at the point of contact (21. 28.). I obtained no satisfactory evidence of any current analogous to (29. 30.), by heating the midlength of a pair of any uncrystallized metal.

31. No evidence, then, appears of any connexion between the currents induced in a mass of bismuth, and those pervading the same metal, when forming part of a circuit. The former seem related to the crystalline laminæ, the latter independent of the laminar direction, and generally analogous to those in other metals, the structure of which is not crystalline.

The changes which take place in zinc may probably offer fairer chance of success (22.); and it is for the purpose of inviting the attention of experimenters to them, that I offer this inquiry to the public in its present stage.

VII. *Of the effects of extraneous contact on a thermo-electric circuit.*

32. It is quoted above from Becquerel (13.), that a continuous circuit of one metal, heated, and touched near the heated point with a cold piece of the same, gives a current from the hot point to the part in contact with the cold piece.

Having placed the thermo-electric pair described (9.) p. 211, in connexion with the magnetest, heat was applied to the middle of the copper leg instead of the twist. This soon communicated, through the good conducting power of the copper, to the point of contact, and proportionate deflection ensued. When the apparatus had cooled, heat was applied to the middle of the iron leg, which did not soon affect the needle. In the meanwhile it was touched, near the heated point, with a piece of iron; then of tinned iron; but still with little effect on the magnetest. Presently the heat was renewed, and a slip of sheet copper substituted for the iron, as a better conductor, being also bent double, to pinch the wire, and thus ensure good contact. A lively deflection now took place, but in direction contrary to that above stated, the current being from the part touched with copper to the point of heat.

33. Instead of the pair, a single wire of tinned iron was now taken, 18 by $\frac{1}{16}$th inches, bent in the form of the letter U, or forming a sort of wire staple. The points of this being set in communication with the magnetest, and the bend, or any part (distant from the mercury *) heated, gave a current, when pinched by copper, near the point of heat, from the part touched to the heated point.

34. A copper staple was now employed, and being pinched with tin, gave the same result; but tin plate being used, instead of tin, gave a current in the opposite direction, *i. e.* from the heated point toward the part touched. Two flakes of antimony, stuck on the ends of a slip of card, produced the same effect as tin plate.

Thus foreign metals, applied to the surface of the wire, affect the circuit. Is the thermo-electric action, then, superficial?

35. A copper staple was taken, one leg of which took three turns round a knob of antimony. Being connected with the magnetest, heat was applied to the copper, just by the antimony: strong deflection took place, as if the antimony had formed part of the circuit, the current flowing from the heated point to the part in contact with the antimony, and this to whichever side the heat was applied. Bismuth being substituted for the antimony, currents took place equally active, but in the converse direction; and the same with tin, when substituted for the bismuth. Hence the inside of a spiral is equally thermo-electric with the outside.

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* Liquid mercury is an active thermo-electric; and care must be taken, wherever it is used for connexion, not to allow any heat to reach the point of contact with it. Many precautions are sometimes requisite to prevent this.

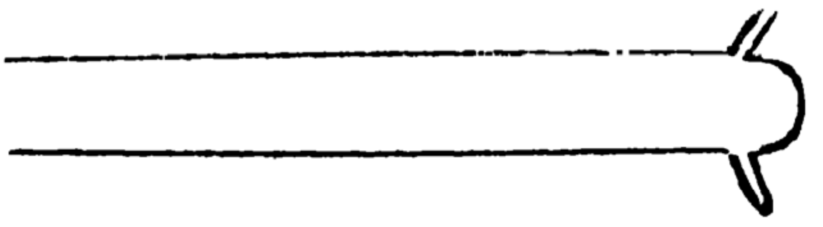
36. A staple was then made, having one leg a tube of tin plate, in which short cylinders of bismuth and tin were made to slide. When either of them approached a heated point in the tube, a current was driven back toward the heated point. Thus it acts inside a tube as well as outside a wire.

37. It is therefore evident that a foreign metal, brought into contact with a homogeneous circuit, near a heated point, enters into the thermo-electric connexion, and determines a current, according to the reaction between the two metals. Such current is, however, weaker than when the second metal intercepts or forms a distinct portion of the circuit, and is what might be expected, regarding the latter only as participating, with the homogeneous metal, in the calorific action on the portion to which it is applied.

38. Becquerel's magnetest was much more delicate than mine, and his results (13.), p. 213, coincide with the property of thermo-negative metals (21.). I have applied this mode of examination to zinc, heating a staple of polished zinc sheet at the bend, and pinching it with another slip from the same sheet, at different distances from the point of heat, consequently at different temperatures. The effects varied with the [illegible] so, apparently, with the direction in which [illegible] ut they were not decided enough to particularize here.

VIII. *In what consists the superior efficacy of tinned surfaces?*

39. It was noticed above (9.) that iron, instead of having its thermo-electric action weakened by tinning the surface, seemed to have it considerably augmented. To place this out of doubt, a staple of iron wire had two loops turned out near



the bend; of these one was tinned, the other left clean, and polished bright. The points also being tinned and connected with the magnetest, the bend was heated nearly to redness, and the loops quickly became hot. The bright loop was then pinched successively with copper, tin, lead, and platinum, but without much effect on the needle. The same metals were then applied to the tinned loop, when the tin gave a deflection of 10°, and the others in proportion.

40. A platinum wire, $12 \times \frac{1}{12}$th inches, was then bent into a staple, and at $\frac{1}{2}$ inch from the bend a bright iron wire, $\frac{1}{16}$ in diameter, was wound 16 turns round it, so as to inclose the platinum in an iron helix for an inch in length. At $\frac{1}{2}$ inch on

the other side of the bend was wound a helix of *tinned* iron wire; the dimensions of the wire and of the helix being the same as the clean iron one. On heating the bend, a deflection of 10° took place, the current setting towards the tinned helix.

41. Equal lengths of tinned and untinned iron wire, of the same dimensions, were twisted together at one end, so as to form a pair. No deflection ensued on heating the twist; but turning the tinned leg round a knob of bismuth, as above (36.), the needle deviated, on heating the twist, 15°; whilst the same knob of bismuth, similarly bound in the clean leg, gave with the same heat only 4° deflection.

42. An iron wire was then cut into lengths of 16 inches, and bent into staples, which were coated as follows:—1, with tin; 2, bismuth; 3, lead; 4, fusible metal, all by heat; 5, with copper, by the humid process. They were heated successively at the bend to 140°, and the forceps (so we may denominate the doubled slip of metal employed to pinch them, as above (32. 33.),) applied at ½ inch from the point of heat. The results are expressed in the following table.

Forceps.

	Tin.	Tinned iron.	Copper.	Iron.
Tin	+15	+8	+12	+5
Fus. metal	+14	+6	+ 6	+5
Lead	+12	0	+ 8	0
Bismuth	+ 4	0	0	−4

These numbers are evidently not precise; but, being the means of several experiments, are indications sufficient for the present purpose.

The sign + signifies that the coated wire was + to the forceps, *i. e.* that the current set from the point of contact toward the point of heat: the sign − indicates, of course, the converse direction. The copper coating differed little from naked iron; the tin exalted the thermo-electric power of the wire; the fusible metal did the same, in an inferior degree; the lead very slightly, if at all; but the bismuth reduced it. At the foot of the last column, iron coated with bismuth was − to bright iron.

43. Similar staples of the same iron wire were bound tightly with lead and tin foils, in single, double, and triple thicknesses. But in every case, and with whatever forceps, the wires so bound were inferior in action to clean iron wires.

The efficacy of copper wire was also increased by tinning and by amalgamation, but in a less degree than that of iron.

44. I am inclined to attribute the efficacy of tinning to the presentation of a soft and clean metallic surface, by which good

metallic contact is facilitated. Tinned iron seems neutral to clean iron when heated together (41.). Fusible metal acts nearly as well as tin. Lead is softer, but much more susceptible of tarnish, and does not take so well upon iron. Bismuth will not yield to pressure, and therefore will not facilitate contact, whilst its strong thermo-negative property can hardly be subdued. The inefficacy of a copper coating is what we should expect from this explanation.

From these experiments it seems probable,—

I. That thermo-electricity differs only in tension from that of the voltaic apparatus, or of the machine.

II. That it is not produced at the expense of caloric.

III. That the radiation of heat, or any kindred property, is not the proximate cause of its development.

IV. That no properties at present known of conduction, either for heat or electricity, are sufficient to account for it.

V. That the metals are resolved by thermo-electricity into two classes: 1. thermo-positive, or becoming electro-positive by heating; 2. thermo-negative, or becoming electro-negative by heating; the first class being always positive to the second when heated: and that their order in their respective classes is dependent on their conductive as well as thermo-electric faculty.

VI. That this property cannot be traced up to, or explained by, the currents produced by heat in single masses of bismuth or other metal.

VII. That foreign metals brought into contact with a homogeneous circuit near the point of heat, participate in the action, and tend to determine the current.

VIII. That the thermo-electric advantage gained to iron and some other metals by tinning, is due to the improved contact, occasioned by the soft and clean metallic surface thus produced.

These results are far from revealing an opening, even in the remote distance, to the theory of the relations between electricity and heat; but if others who have engaged in different branches of the investigation will likewise communicate their observations to the public as they occur, comparison, assisting the details of the note-book, may expedite the pursuit. By affording the means of such comparison, scientific periodicals are most effective agents in the advancement of science; and the general interest is much concerned in their extensive circulation.

XLVII. *Contributions to the Geology of Northumberland and Durham.* *By* N. J. WINCH, *Esq. F.G.S. & A.L.S.*

[Concluded from p. 204.]

Section of the Strata of one of the Pits at Blenkinsop near Toadholes, sunk in the year 1826.

Isabella Pit.

	Fa.	*Yds.*	*Ft.*	*In.*
SURFACE soil, clay, &c.	3	1	0	0
Plate or shale	0	1	2	6
Soft gray slaty sandstone	1	0	1	0
Plate	5	0	2	3
Hard white and yellow slaty sandstone	6	1	0	6
Plate	3	1	1	6
Limestone	4	0	0	0
Plate	4	0	0	3
Coal	0	1	0	5
	29	1	2	5

The pit called the Deep Pit, at Blenkinsop, was 56 fathoms; the upper part all clay, the lower strata corresponding with the above section.

Dip of the coal seam 1 yard in 9 yards.

Regular dip to 10 o'clock A.M.

No whin dykes; there are up and down carts. An out-burst of the coal seam at Little Angerton, in the township of Blenkinsop, 4 feet 6 inches in height. The seam varies from 4 feet 5 inches to 4 feet 8 inches in thickness. The colliery is wrought at present (1830) by a drift or tram-way. Only one workable seam. There is a small seam of crow coal 10 inches thick, but not workable.

I now give a statement of the sinking of another pit at the same colliery (not now at work), called the John Pit.

	Fa.	*Yds.*	*Ft.*	*In.*
Clay	1	1	0	8
Soft blue plate	1	1	1	2
Gray post or girdles	1	0	1	2
Soft gray post ditto with water	2	0	1	0
Soft gray beds	2	1	2	8
Blue plate	3	1	2	0
Limestone*	8	0	2	0
Crow coal	0	0	0	10
Gray thill	0	0	0	8
White post girdles	0	1	1	0
Blue plate	2	0	1	0
Coal	0	1	0	4
	25	0	2	6

---

* A large kibbe or tub of fine lead ore was found in this stratum when sinking the pit.

The average of tonnage per day of the present working pit is 28¼ tons. The annual working in the year 1829 was 2525 score loads, each load containing 4½ imperial bushels.

Seams of coal, inferior in thickness to those belonging to the Newcastle coal formation, and interstratified with the encrinal limestone, as well as with sandstone and shale, are spread over most parts of Northumberland; but owing to these landsale collieries being generally inconsiderable in point of depth and extent of workings, the continuity of the beds of coal has never been accurately ascertained. Sections of coal mines in this formation are to be found in the fourth volume of the first series of the Geological Transactions, p. 60, where an account of Shilbottle Colliery, which supplies Alnwick with fuel, is given: and in the Transactions of the Natural History Society of Newcastle, in vol. i. pp. 126, 127, 128, 129, sections of the more important mines in the vicinity of Berwick-upon-Tweed, are inserted. The section above is of the colliery close to the old castle of Blenkinsop, 33 miles west of Newcastle, and close to the borders of Cumberland. The viewer at this place considered the position of the coal to be below the four-fathom limestone, and above the great limestone of the Alston Moor mining field, and that the bed of coal was the same as that worked in the more extensive mines on Tynedale Fell. From these collieries Carlisle derives its coal. At Angerton, close to Blenkinsop, a very deep quarry is worked in the encrinal limestone before mentioned (see the section below); but I would here remark, that it is next to impossible to trace and identify the various strata of limestones and sandstones to any great distance from the places where they are well known on Alston Moor by the lead miners, for as these beds range towards the north and north-east, they divide and admit others between them; and I have every reason to think that this is even the case with that well-defined rock known by the name of the millstone grit.

Angerton Limestone Quarry, used for Lime, adjoining the present Working Pit (Height thereof) below the Coal.

	Ft.	*In.*
*First bad post or top stone	1	0
*Second ditto	1	0
*Third ditto	1	0
First or good top stone	3	0
*Damp bed	0	8
Second top stone	3	0
Carried forward ...	9	8

	Ft.	In.
Brought forward ...	9	8
*Damp bed	0	6
Thick blue stone	3	0
Small bed above the villies	1	0
*Damp	1	0
First villie	2	0
Second ditto	1	0
Third ditto	1	0
Tweddel's bed	4	0
*Damp	0	6
Three dunny beds, 1 foot 4 inches each	4	0
Three ditto 6 inches each	1	6
*Damp	0	8
Crotly bed	1	2
Curly	3	0
Coarse bed	1	3
Three Leonard beds, 5 inches each ...	1	3
Two true beds, 7 inches each ...	1	2
Middle thick stone	2	6
Yellow or sounding bed	0	8
Moll Harrison	1	3
Thick stone above the bottom	1	3
Cap above ditto	0	6
Bottom stone	2	8
Flag	0	4
	43	2
*Thickness of bad stone, not used, and damps	5	10
Height of stone (or quarry) used for lime	37	4

To the eastward of Blenkinsop, coal is worked at Haltwhistle, Milkbridge, Barkham, Haydon Bridge, Fourstones, opposite Warmley and Acomb or Fallowfield. These places are all in the limestone country, and the coal is thought to be the Blenkinsop seam, though reduced at Milkbridge to nineteen inches in thickness, and at Barkham, at which place the shaft is thirty fathoms and a half deep, from twenty to twenty-five inches. At Hayton Bridge the seam is about twenty inches thick, but at Fourstones it is as thick as at Blenkinsop. The following rough account of the strata sunk through at Acomb was obtained on the spot from an intelligent workman.

	Yds.	*Ft.*
Clay	5	0
Freestone (sandstone)	1	1
Plate (argillaceous shale)	18	0
Plate, white	2	0
Plate	21	0
Brown limestone	7	0
Brown post	7	0
Coal mixed with stone	2	1
	66	2

This may convey some general idea of the rocky beds passed through, but evidently is not to be relied upon like the other sections, which were procured from gentlemen employed in the higher departments of mining.

At Guilsland in the Vale of Irthing, three miles to the west of Blenkinsop, the strata consist of limestone, sandstone, coal, and shale; the latter peculiarly rich in nodules and thin subordinate strata of clay ironstone. One peculiar bed of blue limestone, about half a mile below the Spa, is intimately blended with minute fragments of coal. A seam of coal, two feet thick, crops out in the cliffs on the Wardrew side of the river: it has a good roof of sandstone, and is worked at the surface by the farmers on the estate. Near Baron House, a mile and a half south-west of Wardrew, a seam of coal, three feet eight inches thick, crops out: from its situation, and the inclination of the strata in which it is imbedded, it must be lower in the series than the Blenkinsop seam. The course of the Irthing from Narworth to Guilsland is in the limestone formation; but at Leonard coast the new red sandstone of Cumberland makes its appearance. The limestone has been quarried on the banks of the river a little to the east of the bridge, and the red sandstone not far above it. The Abbey is chiefly built of this material, which, like the same description of stone at Melrose, seems, from its durability, well adapted to the purpose. Immense blocks of fine-grained gray granite are in this neighbourhood scattered over the face of the country, both on the encrinal limestone and on the new red sandstone. To return to the vicinity of Glenwhelt: a mass or irregular bed here crosses the rivulet, and is probably connected on the one hand with the well-known Walltown crags to the eastward, and on the other with the basalt quarried on the top of Windy-law Hill, close to the Carlisle turnpike road, to the westward. In the quarry the basalt assumes a rude columnar structure, and beautiful crystals of purple amethystine quartz are occasionally found in its interstices. In the vicinity of

Thirlwall Castle a bed of encrinal limestone reposes on the basalt, and, as is usually the case when in contact with that rock, has become crystalline in texture, and phosphorescent if laid on red-hot iron: its colour is also changed from dark blue to yellowish white.

Newcastle-upon-Tyne, Dec. 18, 1832.

XLVIII. *On* Bernoulli's *Solution of the Problem of Shortest Twilight. By* T. S. DAVIES, *Esq. F.R.S. L. & E., F.R.A.S., &c.*

[Concluded from p. 185.]

BEFORE we proceed, however, to these supplementary theorems, we may make one or two other remarks upon the result just obtained.

By the theory of equations, if ρ' and ρ'' denote the values of ρ in the last equation, ρ' referring to the minimum, and ρ'' to the maximum cases, we have

$$\cos\rho' \cos\rho'' = \cos^2\lambda \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (10)$$

which is independent of the almacantars between which the star moves. Also taking the difference,

$$\begin{aligned}\cos\rho' - \cos\rho'' &= \cos\lambda\, \frac{\cos^2 \tfrac{1}{2}\,\overline{\rho_{\prime} \backsim \rho_{\prime\prime}} - \cos^2 \tfrac{1}{2}\,\overline{\rho_{\prime} + \rho_{\prime\prime}}}{\cos\tfrac{1}{2}\,\rho_{\prime} \backsim \rho_{\prime\prime}\, \cos\tfrac{1}{2}\,\rho_{\prime} + \rho_{\prime\prime}} \\ &= \cos\lambda\, \frac{\tfrac{1}{2}\{1 + \cos\overline{\rho_{\prime} \backsim \rho_{\prime\prime}} - 1 - \cos\overline{\rho_{\prime} + \rho_{\prime\prime}}\}}{\tfrac{1}{2}(\cos\rho_{\prime} \backsim \rho_{\prime\prime} + \cos\rho_{\prime} + \rho_{\prime\prime})} \\ &= \cos\lambda\, \frac{\cos\overline{\rho_{\prime} \backsim \rho_{\prime\prime}} - \cos\overline{\rho_{\prime} + \rho_{\prime\prime}}}{\cos\rho_{\prime} \backsim \rho_{\prime\prime} + \cos\rho_{\prime} + \rho_{\prime\prime}} \\ &= \cos\lambda \tan\rho_{\prime} \tan\rho_{\prime\prime} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (11)\end{aligned}$$

which, when $\rho_{\prime}$ and $\rho_{\prime\prime}$ (the zenith distances of the almacantars) are taken the complements of each other, the difference of the cosines of the polar distances of the sun for the maximum and minimum time is constant.

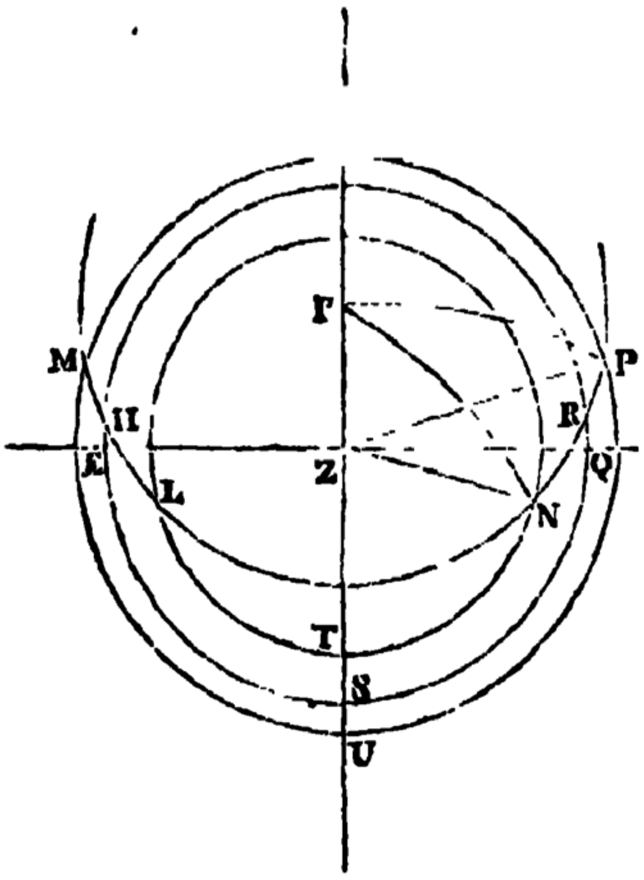
Again, from the same equation (9), we have

$$\cos\rho' + \cos\rho'' = 2\cos\lambda\, \frac{1 + \cos\rho_{\prime} \cos\rho_{\prime\prime}}{\cos\rho_{\prime} + \cos\rho_{\prime\prime}} \quad \ldots\ldots\ldots \quad (12)$$

and by division, we have

$$\frac{\cos\rho'}{\cos\rho''} = \frac{\cos^2 \tfrac{1}{2}\,\overline{\rho_{\prime} \backsim \rho_{\prime\prime}}}{\cos^2 \tfrac{1}{2}\,\rho_{\prime} + \rho_{\prime\prime}} \quad \ldots\ldots\ldots\ldots \quad (13)$$

The question actually resolved is,—what declination of the sun would cause the time of passing from one given almacantar to another to be a maximum or a minimum? The actual path of the sun is no part of the data; and hence D'Alembert's difficulty here arose from *tacitly* introducing a new condition into the *solution* of the problem, which had not been introduced into the process for finding the *equation* of the problem. It is true that under the physical circumstances which were known to exist (but which were unknown in the problem in the aspect under which he viewed it as a mathematician,) there was an obstacle to the maximum answer taking place actually in certain latitudes; but in other given latitudes the maximum might, with the present position of the ecliptic, be actually attained. His showing that the maximum could not be attained in certain particular cases, did not show that it was unattainable in all: and hence his inference, as a general one, that the second answer did not refer to the maximum case of the mathematical problem which he had solved, was in all respects inaccurate. It is important to keep in mind the fact, that the *mathematical* problem which we have *solved* is not always identical with the *physical* problem which we had *proposed*.



Delambre has given as a remarkable property of the azimuths at the beginning and end of twilight, in the case of the minimum, that they are supplementary; but this is not confined to the case in which the horizon is one of the almacantars, as the following investigations show:

1. Let us take $\rho_{,}+\rho_{,,}$ Then

$$\cos PZN = \frac{\cos\rho - \cos\lambda\cos\rho_{,}}{\sin\lambda\sin\rho_{,}}$$

$$= \cot\lambda\,\frac{1+\cos\rho_{,}\cos\rho_{,,}-\sin\rho_{,}\sin\rho_{,,}-\cos^2\rho_{,}-\cos\rho_{,}\cos\rho_{,,}}{\sin\rho_{,}(\cos\rho_{,}+\cos\rho_{,,})}$$

$$= \cot\lambda\,\frac{1-\sin\rho_{,}\sin\rho_{,,}-(1-\sin^2\rho_{,})}{\sin\rho_{,}(\cos\rho_{,}+\cos\rho_{,,})}$$

$$= \cot\lambda\,\frac{\sin\rho_{,}-\sin\rho_{,,}}{\cos\rho_{,}+\cos\rho_{,,}}$$

$$= \cot\lambda\tan\tfrac{1}{2}\,\overline{\rho_{,}-\rho_{,,}} \qquad (14)$$

And in the same way we find

$$\cos PZR = \frac{\cos\rho - \cos\lambda\cos\rho_{\prime\prime}}{\sin\lambda\ \sin\rho_{\prime\prime}} = \cot\lambda\tan\tfrac{1}{2}(\rho_{\prime\prime}-\rho_{\prime})$$

$$= -\cot\lambda\tan\tfrac{1}{2}(\rho_{\prime}-\rho_{\prime\prime}) \ldots (15)$$

And by a comparison of (14) (15) we find at once that PZN + PZR = π, or the azimuths PZN, PZR are supplementary.

2. Take $\rho_{\prime}\backsim\rho_{\prime\prime}$. Then

$$\cos PZN = \frac{\cos\rho - \cos\lambda\cos\rho_{\prime}}{\sin\lambda\ \sin\rho_{\prime}} = \cot\lambda\tan\tfrac{1}{2}(\rho_{\prime}+\rho_{\prime\prime}) \ldots (16)$$

$$\cos PZR = \frac{\cos\rho - \cos\lambda\cos\rho_{\prime\prime}}{\sin\lambda\ \sin\rho_{\prime\prime}} = \cot\lambda\tan\tfrac{1}{2}(\rho_{\prime}+\rho_{\prime\prime}) \ldots (17)$$

Hence in this case the azimuths are equal; or the points in which the sun crosses the two almacantars are in the same vertical great circle,—a remarkable property.

To the case of the shortest twilight problem, the $\rho_{\prime}+\rho_{\prime\prime}$ applies, and gives the same value as found by Delambre and others: but as it depends upon the respective values of $\rho_{\prime}$ and $\rho_{\prime\prime}$ whether the + or $\backsim$ designates the minimum time between the almacantars, we cannot say that the supplemental or the equal relation of the azimuths belongs either to the maximum or to the minimum only.

In the supplementary case we may readily find the difference of the azimuths as follows:—

Let $\xi_{\prime}$ and $\xi_{\prime\prime}$ denote the azimuths: then by the common properties of angles, we have

$$\cos\xi_{\prime}+\cos\xi_{\prime\prime} = 2\cos\tfrac{1}{2}(\xi_{\prime}+\xi_{\prime\prime})\cos\tfrac{1}{2}(\xi_{\prime}-\xi_{\prime\prime}) \ldots\ldots (a)$$

$$-\cos\xi_{\prime}+\cos\xi_{\prime\prime} = 2\sin\tfrac{1}{2}(\xi_{\prime}+\xi_{\prime\prime})\sin\tfrac{1}{2}(\xi_{\prime}-\xi_{\prime\prime}) \ldots\ldots (b)$$

But equation (a) is = 0, and in equation (b) we have

$$2\sin\tfrac{1}{2}(\xi_{\prime}+\xi_{\prime\prime}) = 1, \text{ and hence also}$$

$$-\cos\xi_{\prime}+\cos\xi_{\prime\prime} = 2\sin\tfrac{1}{2}(\xi_{\prime}-\xi_{\prime\prime}) \ldots\ldots\ldots (18)$$

But by (14) (15) we have

$$-\cos\xi_{\prime}+\cos\xi_{\prime\prime} = -2\cot\lambda\tan\tfrac{1}{2}\overline{\rho_{\prime}-\rho_{\prime\prime}}, \text{ and hence}$$

$$\sin\tfrac{1}{2}(\xi_{\prime}-\xi_{\prime\prime}) = -\cot\lambda\tan\tfrac{1}{2}\overline{\rho_{\prime}-\rho_{\prime\prime}} \ldots\ldots (19)$$

In the other case, or that in which azimuths are equal, we have also

$$\sin\tfrac{1}{2}(\xi_{\prime}-\xi_{\prime\prime}) = \cot\lambda\tan\tfrac{1}{2}\overline{\rho_{\prime}+\rho_{\prime\prime}} \ldots\ldots\ldots (20)$$

Delambre, after Cagnoli and some other authors, shows that the angles at the sun are equal in the case of a minimum twilight. They are also equal or supplementary in all cases.

1. Take $\rho_{,}-\rho_{,,}$: then

$$\cos \mathrm{PNZ} = \frac{\cos\lambda - \cos\rho\cos\rho_{,}}{\sin\rho\,\sin\rho_{,}}$$

$$= \frac{\cos\lambda\left\{1 - \dfrac{1+\cos\rho_{,}\cos\rho_{,,}+\sin\rho_{,}\sin\rho_{,,}}{\cos\rho_{,}+\cos\rho_{,}}\cos\rho_{,}\right\}}{\sin\rho\,\sin\rho_{,}}$$

$$= \frac{\cos\lambda\{\cos\rho_{,}+\cos\rho_{,,}-\cos\rho_{,}-\cos^2\rho_{,}\cos\rho_{,}-\cos\rho_{,}\sin\rho_{,}\sin\rho_{,,}\}}{\sin\rho\,\sin\rho_{,}\,(\cos\rho_{,}+\cos\rho_{,,})}$$

$$= \frac{\cos\lambda\,\sin\overline{\rho_{,}-\rho_{,,}}}{\sin\rho\,(\cos\rho_{,}+\cos\rho_{,,})} = \frac{\cos\lambda}{\sin\rho}\cdot\frac{\sin\frac{1}{2}\overline{\rho_{,}-\rho_{,,}}}{\cos\frac{1}{2}\overline{\rho_{,}+\rho_{,,}}} \;\ldots\ldots\ldots\ldots\; (21)$$

In a similar way, we find

$$\cos \mathrm{PRZ} = \frac{\cos\lambda\,\sin\frac{1}{2}\overline{\rho_{,,}-\rho_{,}}}{\sin\rho\,\cos\frac{1}{2}\overline{\rho_{,,}-\rho_{,}}}. = -\frac{\cos\lambda}{\sin\rho}\cdot\frac{\sin\frac{1}{2}\overline{\rho_{,}-\rho_{,,}}}{\cos\frac{1}{2}\overline{\rho_{,}+\rho_{,,}}} \ldots (22)$$

And it follows at once that $\mathrm{PNZ}+\mathrm{PRZ} = \pi$, or the angles at the sun are supplementary of one another.

2. Take $\overline{\rho_{,}+\rho_{,,}}$: then,

$$\cos \mathrm{PNZ} = \frac{\cos\lambda\left\{1 - \dfrac{1+\cos\rho_{,}\cos\rho_{,,}-\sin\rho_{,}\sin\rho_{,,}}{\cos\rho_{,}+\cos\rho_{,,}}\cos\rho_{,}\right\}}{\sin\rho\,\sin\rho_{,}}$$

$$= \frac{\cos\lambda}{\sin\rho}\cdot\frac{\sin\frac{1}{2}\overline{\rho_{,}+\rho_{,,}}}{\cos\frac{1}{2}\overline{\rho_{,}-\rho_{,,}}} \;\ldots\ldots\ldots\ldots\ldots\; (23)$$

And in the same way, we find

$$\cos \mathrm{PRZ} = \frac{\cos\lambda}{\sin\rho}\cdot\frac{\sin\frac{1}{2}\overline{\rho_{,,}+\rho_{,}}}{\cos\frac{1}{2}\overline{\rho_{,,}-\rho_{,}}} \;\ldots\ldots\ldots\ldots\ldots\; (24)$$

Hence, since $\cos\frac{1}{2}\overline{\rho_{,}-\rho_{,,}} = \cos\frac{1}{2}\overline{\rho_{,,}-\rho_{,}}$, these angles are equal.

We may here remark, that Delambre's proof is insufficient to justify his inference of the equality of these angles, in the case he has considered. His argument is,

$$\text{since } \sin \mathrm{ZNP} = \sin \mathrm{PZN}\,\frac{\sin\lambda}{\sin\rho},$$

$$\sin \mathrm{ZRP} = \sin \mathrm{PZN}\,\frac{\sin\lambda}{\sin\rho},$$

$$\text{and } \sin \mathrm{PZN} = \sin \mathrm{RZN};$$

hence also $\mathrm{ZNP} = \sin \mathrm{ZRP}$, and *therefore* the angles ZNP, ZRP are themselves equal. But manifestly *this does not show* *that they might not be supplemental instead of equal.* Hence his deduction is not authorized. It is, however, true, as may be seen by putting the corresponding values $\frac{\pi}{2}$ and $\frac{\pi}{2} + 2a$ for $\rho_{,}$ and $\rho_{,,}$ in the above equations: *vide* Leybourn's Edition of the Diaries, *ut supra*, or Delambre's *Histoire de l'Astronomie Ancienne.*

It has been objected to the solution of Bernoulli, by Delambre, that it does not give the time of duration of the shortest twilight, but the declination of the sun when the twilight is shortest. By referring, however, to Bernoulli's own words, we shall find that he proposed to determine "*le jour* de plus petit crépuscule," and not to find the *duration* on that day. As an objection, then, Delambre's amounts to this—that Bernoulli, having proposed to himself one problem, did not substitute the solution of another *instead* of the one he had proposed! This is travelling out of the way to find imperfections, most assuredly. It would, indeed, at first sight seem difficult to account for such an insignificant objection being urged at all; but doubtless it originated in the somewhat exaggerated expressions of admiration bestowed upon Bernoulli's solution by Montucla, and the fact of the historian of mathematics having greatly undervalued the original solution of Nunez. It is a fact, however, that cannot have escaped the notice of every one who has had occasion to consult Delambre's Histories, that there is a feeling not altogether friendly displayed by him towards both Montucla and Bailly; and that points of comparative insignificance are often dwelt upon at very incommensurable length, where he has discovered, or believes he has discovered, those authors to be in error. The present is one of them. Bernoulli did not, indeed, solve the other problems that were connected with the one he chose to attempt; but having solved that one, all the others might have been followed out without difficulty,—of which Bernoulli, of course, was well aware. Nor is it more difficult in the general case.

$$\text{For } \cos(\xi_{,}-\xi_{,,}) = 1 - 2\sin^2\tfrac{1}{2}(\xi_{,}-\xi_{,,})$$

$$= 1 - 2\cot^2\lambda\tan^2\tfrac{1}{2}\,\overline{\rho_{,}-\rho_{,,}}$$

But taking RN as the arc of a great circle joining the points R, N, we have $\cos \mathrm{RN} = \cos\rho_{,}\cos\rho_{,,} + \sin\rho_{,}\sin\rho_{,,}\cos(\xi_{,}-\xi_{,,})$

$$\sin^2\tfrac{1}{2}(\theta_{,,}-\theta_{,}) = \frac{1-\cos \mathrm{RN}}{2\sin^2\rho}$$

$$= \frac{1-\cos\rho_{,}\cos\rho_{,,}-\sin\rho_{,}\sin\rho_{,,}+2\sin\rho_{,}\sin\rho_{,,}\cot^2\lambda\tan^2\tfrac{1}{2}\,\overline{\rho_{,}-\rho_{,,}}}{2\sin^2\rho}$$

$$= \frac{1-\cos \overline{\rho_{\prime} \backsim \rho_{\prime\prime}} + 2 \sin \rho_{\prime} \sin \rho_{\prime\prime} \cot^2 \lambda \tan^2 \tfrac{1}{2} \overline{\rho_{\prime} - \rho_{\prime\prime}}}{2 \sin^2 \rho}$$

$$= \frac{\sin^2 \tfrac{1}{2} \overline{\rho_{\prime} \backsim \rho_{\prime\prime}} + \sin \rho_{\prime} \sin \rho_{\prime\prime} \cot^2 \lambda \tan^2 \tfrac{1}{2} \overline{\rho_{\prime} - \rho_{\prime\prime}}}{\sin^2 \rho}$$

$$= \frac{\tan^2 \tfrac{1}{2} \overline{\rho_{\prime} \backsim \rho_{\prime\prime}}}{\sin^2 \lambda \sin^2 \rho} \left\{ \cos^2 \tfrac{1}{2} \overline{\rho_{\prime} - \rho_{\prime\prime}} \sin^2 \lambda + \cos^2 \lambda \right\}$$

$$= \frac{\tan^2 \tfrac{1}{2} \overline{\rho_{\prime} \backsim \rho_{\prime\prime}}}{\sin^2 \lambda \sin^2 \rho} \left\{ 1 - \sin^2 \lambda \sin^2 \tfrac{1}{2} \overline{\rho_{\prime} - \rho_{\prime\prime}} \right\} \cdots\cdots\cdots\cdots \quad (25)$$

Substituting in (25) the values of ρ adapted to the two cases of $\rho_{\prime} \backsim \rho_{\prime\prime}$ and $\rho_{\prime} + \rho_{\prime\prime}$, we shall have expressions for the corresponding times. It is altogether unnecessary to consider them here, or to enter upon a discussion of any other analogies between the properties, which Delambre has collected together, of the diagram employed, and those which belong to the more general problem discussed in this paper; since they do not all hold true, and where they do hold true they are mere matters of mathematical curiosity,—at the same time that they offer no real difficulties to any one who may be tempted to enter upon their investigation.

Bath, August 8, 1833.

⁂ Mr. Skene, in the prefatory remarks to his solution, mentions an analytical one by Fontana; but he does not say whether it was a separate publication or inserted in any of the foreign scientific Transactions. I have sought it without success. A solution by means of the Stereographic Projection of the figure was given by M. Dandelin of Liege, in vol. ii. of Quetelet's *Correspondance Mathématique et Physique*; but it offers nothing novel in principle to call for special remark. Several neat solutions may also be seen in the Gentleman's Diary for 1817; but they also differ more in form than in principle, from others which had been previously published.

XLIX. *On a Difficulty in the Theory of the Attraction of Spheroids.* By J. MacCullagh, *F.T.C.D.*[*]

AN approximate theorem discovered by Laplace, and relating to the attraction of a solid slightly differing from a sphere on a point placed at its surface, has given rise to many disputes. It has engaged the attention of Lagrange, Ivory, and Poisson*. I hope the following remarks will make the matter clear.

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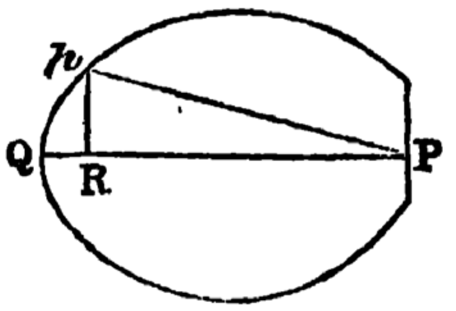
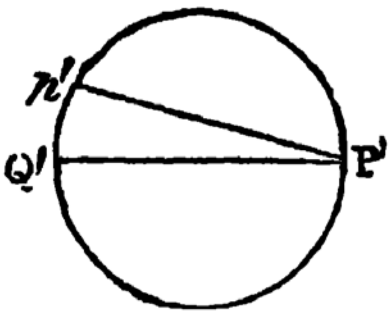
[*] Communicated by the Author.

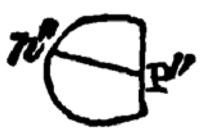
Let us consider the function which expresses the sum of every element of a solid divided by its distance from a fixed point, and let us denote it, as Laplace has done, by the letter V. It is necessary to find the value of V for a pyramid of indefinitely small angle, the fixed point being at its vertex. Calling φ the small solid angle of the pyramid (or the area which it intercepts on the surface of a sphere whose radius is unity and centre at the vertex), it is manifest that the element of the pyramid at the distance r from the vertex is $\phi r^2 dr$; dividing, therefore, by r, and integrating, we have $\frac{1}{2}\phi r^2$, or ϕ multiplied into half the square of the length, for the value of V.

Again, supposing the force to vary inversely as the square of the distance,—the only hypothesis which can be of use in the present inquiry,—the attraction of the same pyramid on a point at its vertex, and in the direction of its length, is manifestly equal to φr.

Let us now consider a solid of any shape, regular or irregular, terminated at one end by a plane to which the straight line PQ is perpendicular at the point P; and let there be a sphere of any magnitude, whose diameter P′Q′ is parallel to PQ. Let P″ be a fixed point, and from the points P, P′, P″, draw three parallel straight lines Pp, P′p', P″p'', the first two terminated by the surfaces of the solid and of the sphere, the third P″p'' in the same direction with them and equal to their difference, without regarding which of them is the greater, and suppose all the points p'' taken according to the same law, to trace the surface of a third solid. Let Pp, P′p'; P″p'', be edges of three small pyramids with their other edges proceeding from P, P′, P″, parallel, and having of course the same solid angle which we shall call φ, denoting by r, r', r'', their respective lengths, and by V, V′, V″, the values of the function V for each of them. Drawing pR perpendicular to PQ, the attraction of the pyramid Pp in the direction of PQ will be







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* See Pontécoulant, vol. ii. p. 380; Foreign Quarterly Review, vol. v. p. 248,

equal to $\phi \times \mathrm{PR}$; call this attraction A, and let a be the radius of the sphere.

Since r'' is the difference of r and r', we have $r^2 + r'^2 - r''^2 = 2rr' = 2\,\mathrm{PR} \times \mathrm{P'Q'}$, and multiplying by $\frac{1}{2}\phi$ we find $\frac{1}{2}\phi r^2 + \frac{1}{2}\phi r'^2 - \frac{1}{2}\phi r''^2 = 2a\phi \times \mathrm{PR}$, that is $\mathrm{V} + \mathrm{V}' - \mathrm{V}'' = 2a\mathrm{A}$. The same thing is true for any other three pyramids similarly related to each other, throughout the whole extent of the three solids which are exhausted by them at the same time; and hence, if we now denote by V, V′, V″, the *whole* values of the function V for the three solids, and by A the *whole* attraction of the first of them parallel to PQ on a point at P, we shall still have $\mathrm{V} + \mathrm{V}' - \mathrm{V}'' = 2a\mathrm{A}$.

To express this general theorem in the notation of Laplace, we have merely to observe that the attraction A is synonymous with $-\left(\frac{d\mathrm{V}}{dr}\right)$, and that the quantity V′ for the sphere is equal to $\frac{4}{3}\pi a^2$. Substituting these values, we find

$$\mathrm{V} + 2a\left(\frac{d\mathrm{V}}{dr}\right) = -\tfrac{4}{3}\pi a^2 + \mathrm{V}'';$$

an exact equation, differing from the approximate one of Laplace only in containing the quantity V″, and totally independent of the nature of the surface or the magnitude of the sphere, the only things supposed being that all the lines drawn from P meet the surface again but once, and that no part of it passes beyond a plane through P at right angles to PQ.

With respect to the limit of the quantity V″, it is obvious that if a hemisphere be described from P″ as a centre with a radius equal to the greatest difference δ between the lines Pp, P′p', the solid P″p'' will lie wholly within this hemisphere, and consequently V″ will be less than the value of V for the hemisphere, that is, less than $\pi\delta^2$; for here all the little pyramids from the centre have the same length δ, and their bases are spread over the hemispherical surface; wherefore $\mathrm{V}'' = 2\pi \times \frac{1}{2}\delta^2 = \pi\delta^2$. All this is independent of anything but the suppositions just mentioned.

If now PQ be supposed to be a spheroid of any sort, slightly differing from the sphere P′Q′, and such that the line PQ, perpendicular to the surface at P, passes nearly through the centre, then all the differences, of which δ is the greatest, being of the first order, the quantity V″, which is less than $\pi\delta^2$, will be of the second order; and therefore neglecting, as Laplace has done, the quantities of that order, we get the theorem in question.

It may be well to apply the general theorem to the simple case in which the first solid is a sphere of the radius a', because both Lagrange and Ivory have used it to show that the reasonings of Laplace are incorrect. In this case, then, the surface described by the point p'' is that of a sphere whose radius is the difference between a and a'; and the values of V, V′, V″ and A, are $\frac{4}{3}\pi a'^2$, $\frac{4}{3}\pi a^2$, $\frac{4}{3}\pi (a'-a)^2$, and $\frac{4}{3}\pi a'$, respectively.

Substituting these values in the equation $V+V'-V''=2aA$, and omitting the common factor $\frac{4}{3}\pi$, the resulting equation

$$a'^2+a^2-(a'-a)^2=2aa$$

ought to be identical;—and so it manifestly is.

L. *On a New Principle in Statics, called the* PRINCIPLE OF LEAST PRESSURE. *By the Rev.* H. MOSELEY, *B.A. Professor of Natural Philosophy in King's College, London*[*].

LET there be conceived a system of forces, of which a certain number are given in magnitude and direction, and the rest are supplied by the resistances of as many fixed points.

Also let the points of application of all the forces of the system be supposed to be given.

Now to the complete determination of any force in magnitude and direction, its point of application being given, it is necessary that we know its resolved parts in the directions of three rectangular axes.

To ascertain, therefore, completely, the magnitudes and directions of the resistances on the different fixed points of the system we have supposed, we must have at least three times as many equations between the resolved parts of the forces and resistances which compose it, as there are points of resistance.

The known conditions of equilibrium supply, at most, but six such equations. If there be more than two points of resistance, these equations are therefore insufficient. And it remains to establish some other relation between the resistances, their several points of application, and the other forces of the system, as shall enable us to determine the former, in terms of the others, of which they are manifestly functions.

The following principle is sufficient for this determination. It is believed to constitute a new principle in statics. *If there be any number of forces in equilibrium among which there enters a system of resistances, then are these resistances such, that their sum is a* MINIMUM; *each being considered a function of the*

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[*] Communicated by the Author.

coordinates of its point of application, taken with a positive sign, and subjected to the conditions imposed by the equilibrium of the whole.

Let A designate the *given* forces of the system, B the resistances, and C *any* other system of forces which being applied to the same points with the forces of the system B, would maintain the equilibrium. Also let the system C be supposed to replace the system B.

Now each force of the system C, under these circumstances, *just* sustains and is equivalent to the pressure propagated to its point of application by the forces of the system A; or it is equivalent to that pressure *together with* the pressure propagated to its point of application by the other forces of the system C.

In the former case, it is identical with the corresponding *resistance* of the system B. In the latter case it is *greater* than it.

The sum of the forces of the system B, each being considered *a function of the coordinates of its point of application, &c. &c.*, is therefore a minimum.

Let P represent any force of the system B; x, y, z its coordinates; and α, β, γ the angles it makes with the axes of these, respectively. Let M_1, M_2, M_3 represent the sums of the resolved parts of the forces of the system A, and N_1, N_2, N_3, the sums of their moments;

$$\therefore \left.\begin{aligned} \Sigma P \cos\alpha &= M_1 \\ \Sigma P \cos\beta &= M_2 \\ \Sigma P \cos\gamma &= M_3 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots \quad (1)$$

$$\left.\begin{aligned} \Sigma P\,(y \cos\alpha - x \cos\beta) &= N_1 \\ \Sigma P\,(x \cos\gamma - z \cos\alpha) &= N_2 \\ \Sigma P\,(z \cos\beta - y \cos\gamma) &= N_3 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (2)$$

Also $\alpha_1, \beta_1, \gamma_1, \alpha_2, \beta_2, \gamma_2$, &c. representing the values of α, β, γ, for different points of the system B, we have

$$\left.\begin{aligned} \text{Cos}^2\,\alpha_1 + \cos^2\beta_1 + \cos^2\gamma_1 &= 1 \\ \text{Cos}^2\,\alpha_2 + \cos^2\beta_2 + \cos^2\gamma_2 &= 1 \\ \text{\&c.} \qquad \text{\&c.} \qquad &= \end{aligned}\right\} \quad \ldots\ldots \quad (3)$$

Now the relations existing by reason of the nature of the system between the coordinates of the several points of application of the forces P may be expressed by the equations

$$\left.\begin{aligned} u_1 &= 0 \\ u_2 &= 0 \\ u_3 &= 0 \\ \text{\&c.} &= 0 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (4)$$

Also $$\Sigma P = \text{minimum} = V \quad \ldots\ldots\ldots\ldots\ldots \quad (5)$$

Differentiating the above equations, P being considered a function of x, y, z; multiplying the differentials of equations (1) by the indeterminate quantities A_1, A_2, A_3, respectively,

(2) by B_1, B_2, B_3,
(3) by μ_1, μ_2, μ_3, μ_4, &c. &c. &c.,
(4) by λ_1, λ_2, λ_3, λ_4, &c. &c. &c.,

and adding the resulting equations (all whose right-hand members disappear) to the differential of equation (5), which is

$$\Sigma \left\{ \frac{d\,\mathrm{P}}{d\,x}\,\delta\,x + \frac{d\,\mathrm{P}}{d\,y}\,\delta\,y + \frac{d\,\mathrm{P}}{d\,z}\,\delta\,z \right\} = \delta\,\mathrm{V},$$

we shall obtain an equation in which the coordinates of the several points of resistance x_1, y_1, z_1, x_2, y_2, z_2, &c. &c., and α_1, β_1, γ_1, α_2, β_2, γ_2, &c. &c., may be considered as independent variables; so that by the condition $\mathrm{V} =$ a minimum, the coefficients of $\delta\,x$, $\delta\,y_1$, $\delta\,z_1$, $\delta\,x_2$, $\delta\,y_2$, $\delta\,z_2$, &c. &c. $\delta\,\alpha_1$, $\delta\,\alpha_2$, &c. $\delta\,\beta_1$, $\delta\,\beta_2$, &c. $\delta\,\gamma_1$, $\delta\,\gamma_2$, &c. &c., respectively equal nothing. We thus obtain, in respect to each point of resistance, six equations, reduced to four, by the elimination of the indeterminate quantities λ and μ, which are different for the different points of resistance. Of these four equations, two, involving partial differential coefficients of P, from which one of the variables x, y, z may be eliminated by means of the equation $u = 0$, are equivalent to one complete differential equation between the other two variables, so that on the whole there are three equations determining the quantities P, α and β in terms of the coordinates x,y,z, and the indeterminate quantities A_1, A_2, A_3, B_1, B_2, B_3 (which are the same for all the different points of resistance), whilst these are in their turn determined, by substituting the resulting values of P_1, α_1, β_1, P_2, α_2, β_2, &c. in the equations (1) and (2).

Thus the values of P, α, and β, that is, the amount and direction of the pressure upon any fixed point of the system, are completely determined.

The various analytical operations indicated above, are in their nature too elaborate to be *here* brought under the eye of the reader. Those conversant with the methods of analysis will, however, readily supply the deficiency.

It may be observed that in the case in which the points of resistance are all in the same plane*, the pressure upon any point whose coordinates in that plane are x and y, is ascertained to come under the formula

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* This particular case of the general proposition has formed the subject of a paper by the illustrious Euler, entitled, "*De Pressione Ponderis in Planum cui incumbit,*" Mem. Ac. Pet. Novi Commentarii, vol. xviii. His discussion of the question is most ingenious and elaborate. It is impossible, however, to admit the *hypothesis* on which he has grounded it.

$$\frac{C}{1 + Ax + By},$$

A, B and C being constants, dependent upon the number and positions of the points of support, and upon the forces impressed upon the system.

From this formula it follows, that there is a certain line, distant from the origin by a quantity equal to $\frac{1}{\sqrt{A^2 + B^2}}$, and inclined to the axis of x at an angle whose tangent is $\frac{A}{B}$; about which the moments of all the pressures are the same. This line may be properly called the *axis of least pressure*, as the principle whence its existence and properties have been deduced, may be designated the *principle of least pressure.*

Where the points of support are in the same line, this axis resolves itself into a point; and it follows that the moments of the resistances about a certain point in the line in which they act, are all equal;—a result which is verified by the known conditions of the pressure upon two points of support.

LI. *Notice respecting certain Changes of Colour in the Choroid Coat of the Eyes of Animals. By Sir* David Brewster, *LL.D. F.R.S.*

IN the Number of this Journal for August last, (p. 87) Mr. Fielding has published some interesting experiments respecting certain changes of colour induced by chemical and other agents on the membrane lining the choroid coat of the eye; and he has particularly described an experiment which shows that when the colours have disappeared by drying, they may be revived by simple immersion in water.

In the chapter on the colours of natural bodies in the article Optics in the Edinburgh Encyclopædia, I have brought forward this fact as illustrating and supporting Sir Isaac Newton's Theory of the Colours of Natural Bodies, and I am induced to mention this at present, not only for the purpose of giving to Dr. Drummond of Belfast, the merit of having first made this curious experiment, but also of making some further observations upon it in reference to its connexion with the theory of Newton. The following is the passage in which it is mentioned.

" Dr. Drummond of Belfast observed, that the *membrane* behind the retina of the dog, and other animals which produce those *blue* and *green* and sometimes *red* reflections, which often shine with such brilliancy in the living animal, loses the power of reflecting these tints when it is dry, and becomes entirely black. Upon learning this fact from Dr. Drummond, we prepared several eyes that reflected these colours with great vivacity, and invariably found that they became *black* when *dry*, and *blue* and *green* when softened with water. After some of them had remained *four or five years in a dry state*, they still possessed the property of developing their colours by moisture. It is worthy of remark, that the *black* passed instantly into a brilliant *blue*, the *blue* into *green*, and the *green* into a *greenish yellow**."

After this paragraph was written, I had occasion to repeat the experiment on one of the eyes just mentioned, and I found that the colours could be revived in the membrane ten or twelve years after it had been taken from the animal.

I do not recollect to have anywhere seen it stated that the brilliant colours which appear in the eyes of animals have been noticed in the human eye; but I had occasion many years ago to observe them in the most distinct manner, and to examine them repeatedly in the eye of a boy about ten years of age. The colour was a *bright red*, with a purplish tinge; but it is not in my power to ascertain now whether or not this colour varied with an increase of years. On this point Mr. Fielding remarks, "As regards the human eye, I have had very little opportunity for investigation; and though I have proved its [the new membrane's] existence, *I cannot say that it ever presented any distinctly coloured appearance.*"

Having just succeeded in finding one of the prepared specimens of the tapetum of an ox's eye, mentioned in a preceding paragraph, and which has been preserved for nearly twenty years, I find that when dry, it is as black as charcoal; and that the blue and green colours of the membrane above referred to, could be revived in all their original brightness by immersion in water.

It is a curious circumstance in the colours thus produced, that though they are apparently those of thin plates, they advance immediately from black to blue and green of the second order, all the intermediate colours of the first order being omitted. The same phænomenon occurs in the peacock's tail, in the plumage of different birds, and in Labrador felspar. In another communication I hope to be able to give a satisfactory explanation of this remarkable interruption of continuity.

Belleville, by Kingusie, Sept. 18, 1833.

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* Edinb. Encycl. vol. xv. p. 623.

LII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

AGREEABLY to the intention expressed in our last Number, we now give abstracts of two of the most important communications read on June 14th.

Notice of the Elliptic Orbit of ξ *Boötis*, with a further Approximation to the Orbit of γ *Virginis*. By Sir John Herschel.

The elements of the orbit of ξ *Boötis* are given for the first time; those of γ *Virginis* are recalculated by the aid of observations made in the current year, by Sir J. Herschel and others, which differ materially from the Ephemeris published in tho Supplement to the Nautical Almanac for 1832. Comparisons of the calculated results, with observations, are given in both cases.

The elements are as follow :—

ξ *Boötis.*

Semiaxis major……… $a = 12''{\cdot}56$
Excentricity……… $e = 0\ {\cdot}59374$
Pos. of perihelion…… $\varpi = 138° 24'$
Angle between lines of nodes and apsides …… $\lambda = 100° 59'$
Inclination to the plane of the sky $\gamma = 80° 5'$
Position of node … ☊ $= 359° 59'$
Period in tropical years……… $P = 117{\cdot}14$
Mean motion……… $n = -3°{\cdot}0733$

Perihelion passage, Dec. 17, 1779.

"These elements are interesting in several respects. In the first place, the period they indicate fills up an intermediate and wanting link between the short periods of η *Coronæ*, ξ *Ursæ*, &c., and the much longer ones of *Castor*, σ *Coronæ*, 61 *Cygni*, γ *Virginis*, &c. establishing (so far as we can at present rely on such determinations,) a connected scale of periods, from less than half a century to upwards of six centuries. The great amount of the excentricity of the orbit in question strengthens the induction which already begins to assign, in no unequivocal manner, orbits rather of a cometary than a planetary character to the binary stars generally. This orbit, moreover, is the most oblique to the line of sight which has yet been calculated; and as the apparent position of the projected perihelion is not very far from making a right angle with the line of nodes, the longer axis of the real ellipse is violently fore shortened, and the whole character of the apparent ellipse completely distorted, as seen in projection, from what it is in reality."

γ *Virginis.*

Major semiaxis……………………… $a = 12''{\cdot}090$
Excentricity……………………… $c = 0{\cdot}8335$
Perihelion projected ……………… $\varpi = 36° 40'$
Perihelion from node on the orbit …… $\lambda = 282° 21'$
Inclination to heavens……………… $\gamma = 67° 2'$
Node……………………… ☊ $= 97\ 23$
Period in tropical years ……………… $P = 628{\cdot}90$
Mean annual motion ……………… $n = -0°{\cdot}57242$
Perihelion passage ……………… $\tau = 1834{\cdot}63$

On the Adoption of a Standard of Optical Power by Observers. By the Rev. W. R. Dawes.

"Almost from the first invention of telescopes to the present day, the eclipses of *Jupiter's* satellites have afforded matter of interest to the astronomer, and to all who are anxious for the improvement of geographical science. Unhappily, however, notwithstanding all which has been done to improve this most simple method of determining differences of terrestrial longitude, there are few observations whose results are less satisfactory. Nor will this excite surprise, if it be considered, that the instant when any of these phænomena is observed is greatly influenced by the circumstances under which it is witnessed. When observed at different places, the results may be affected by diverse states of the atmosphere, and altitudes of the planet; besides the varieties existing in the *instruments* employed, and the *eyes* which use them. But though it is beyond our power to control the state of the atmosphere, or to alter the situation of the planet, we may, I conceive, do much to remove the enormous discrepancies arising from the differences in the optical power and acuteness which are brought to bear on this interesting class of phænomena.

"It might be supposed, perhaps, that a sufficient agreement of optical power might be obtained by fixing on a certain aperture, and a suitable magnifying power, as a standard to be adopted by all observers who are desirous of determining differences of longitude by means of these eclipses. And, no doubt, a much nearer approach to what is desirable would be thus obtained, than can be expected while some are employing telescopes possessed of immensely greater illuminating power than those used by others for the same purpose. However important in other respects observations may be, made in well determined stations, with large instruments, they are obviously not comparable with the results obtained by such instruments as are usually possessed by private individuals of astronomical taste, and to whom this species of observation is commonly very interesting, and would be more so were its results less unsatisfactory. Yet, were we to agree to cut down the larger instruments to a certain aperture, we should still leave two sources of disagreement unprovided against, which however are within our control. First, the instruments thus *apparently* placed on a footing would differ much in excellence, and consequently in the time when they would render an immerging or emerging satellite only just visible. Secondly, even could this point be adjusted, there would remain the very great variety which exists in different individuals in respect of *acuteness of vision*. This I believe to be much greater than is generally supposed; for I know it to be such, in some instances, as to render the 42-inch achromatic telescope as efficient an instrument to one observer in discerning minute points of light, as a 5-feet achromatic is to another; each instrument having its usual aperture, and being of corresponding excellence. In further proof of this, I may advert to the fact, that in every instance in which Captain Smyth and myself have witnessed the same *emersion*, the satellite appears to have been first discerned by me;

the instrument used at Bedford being, as I understand, the excellent 8½-feet achromatic of nearly 6 inches aperture, and consequently possessing an illuminating power of between two and three times greater than that of my 5-feet achromatic. Hence it is manifest that some plan must be devised for equalizing the optical powers of the different *observers and their instruments*, the *eye* and the *telescope* being viewed as one compound optical machine. And this being attained, the varying condition of the atmosphere would alone be uncontrollable; for I imagine the differences of the planet's altitude might be very nearly allowed for by a corresponding alteration in the aperture or power generally used.

" I would therefore earnestly recommend the adoption of some celestial object as a *standard* of the optical power to be employed; and I know of none possessing so many advantages as the minute companion of *Polaris.* It is readily found, is visible throughout the year, and preserves nearly the same altitude. Let each observer then ascertain, by a series of experiments on several clear nights, what is the *minimum aperture* with which his instrument will enable him, on an average of such nights, to keep this delicate object *just steadily* in view. Perhaps it would be proper to fix the power employed at about 80. This power is very usually attached to the 3½-feet achromatic, the aperture of which instrument will, I imagine, to most eyes, admit of a little diminution to bring it to the proposed standard. This telescope is, moreover, not an uncommon one among observers; yet I am disposed to regret, that the object I have selected as on the whole the best, is not sufficiently visible with the usual 30-inch achromatic. For, the greater the number of observers engaged, the more interesting and useful are the results likely to prove.

" It appears to me, that by the adoption of this plan, all the observers of these phænomena would be placed as nearly as possible on an equal footing, and every source of uncertainty within our control would be removed. I am not, indeed, prepared to suggest precisely the best mode of meeting the differences of the planet's altitude, as seen from different places; but this will not vary enormously in the British Isles; and if the plan should meet with the approbation of observers within these limits, and by them be subjected to a fair trial during the next apparition of *Jupiter*, further details and improvements may be brought forward, should the method be thought worthy of general adoption.

" It would, of course, be highly desirable that the same mode of observation should be pursued (in respect at least of *one* instrument and observer) at the principal observatories, whose longitude is accurately settled, as the results thus obtained would have great influence in determining the question of its practical utility.

" I need not dwell on the importance of distinctly specifying the *atmospheric circumstances* under which these observations are made, as otherwise it will be impossible to judge of their comparability. It might also conduce to further improvement of the method, if the planet's *hour-angle* or *estimated altitude* were recorded, and it would be convenient were the same *denomination* of time employed in registering the observations; or if both the sidereal and mean time were given, as in the Greenwich Observations.

"Should this subject be found to possess sufficient interest to engage a few careful observers in thus watching for the eclipses of *Jupiter's* satellites during the present year, the results might be communicated to the Society, so as to appear in one of their Monthly Notices during the session of 1834; and thus evidence would be had, at one view, of the degree of advantage to be expected from the adoption of such a plan of simultaneous and uniform observation."

ZOOLOGICAL SOCIETY.

June 11.—A specimen of the *Patagonian Penguin, Aptenodytes Patachonica*, Gmel., recently presented to the Society by Lady Rolle, was exhibited. Mr. Yarrell availed himself of the opportunity to point out on it the proofs which it afforded of the statement made by him at the Meeting on March 12, that the *woolly Penguin* of Dr. Latham is the young condition of this species.

A specimen was exhibited of a *Goose* from the Sandwich Islands, being one of a pair recently living at the Society's Gardens, to which they were presented by Lady Glengall. Mr. Vigors characterized it as a species of *Barnacle Goose*, by the name of *Bernicla Sandvicensis*, and pointed out its distinguishing marks. He also observed on the general resemblance in the distribution of colouring which occurs in the species of *Bernicla* and in those of many other groups of *Birds*.

Numerous skins of *Birds* were exhibited, which had recently been obtained by the Society from California. They formed part of the collection, the *Mammalia* of which were brought under the notice of the Society by Mr. Bennett on March 26 (see page 66). Mr. Vigors remarked on them generally as regarded the geographical distribution of many of them; and pointed out, as apparently hitherto undescribed, an *Ortyx*, a *Falco*, two species of *Coccothraustes*, and a *Psittacara*. Among the known birds were several of those first described by Mr. Swainson in the 'Fauna Boreali-Americana,' and a specimen of *Ortyx Montezumæ*, Vig.

Dr. Grant directed the attention of the Meeting to a fine entire skull of the *round-headed Grampus*, (*Delphinus globiceps*, Cuv.,) from the North Pacific Ocean, presented to the Society by Capt. Delvitte, R.N., Corr. Memb. Z.S. He availed himself of the opportunity of entering into some details regarding the osteology of the head of the *Grampus* and other predaceous *Cetacea*.

Specimens were exhibited of two *Monkeys*, forming part of the Society's Museum, which Mr. Bennett characterized as SEMNOPITHECUS *Nestor* and CERCOPITHECUS *pogonias*. The former may be assumed to be a native of India; the latter is from the vicinity of Fernando Po.

A specimen was exhibited of the *black Lemur, Lemur niger*, Geoff., which had recently been added to the Society's Menagerie. In calling the attention of the Society to it, Mr. Bennett stated his belief that this was the first individual of the species which had fallen under the observation of zoologists since the days of Edwards, its original describer, who saw and figured one which was living in 1755 in the possession of a surgeon in London. The description and figure given by Edwards have consequently been hitherto the only proofs of the existence of such an animal. Mr. Bennett added that the *black Lemur* is the type of the *Lemur Macaco*, Linn.; and that the *Vari*, to which the name of *Lem. Macaco* has been applied by modern authors, is given by Linnæus as the Var. d. of that species. Custom having, however, transferred the specific name to the variety, he deemed it better to acquiesce in the use which has obtained, leaving to the *Vari* the name of *Lem. Macaco*, and to the *black Lemur* that of *Lem. niger.*

Specimens were exhibited of various *Mammalia*, *Birds*, and *Reptiles*, from the continent of India, which had been recently presented to the Society by Thomas Heath, Esq. Mr. Bennett observed on the several objects, pointing out especially the more interesting among them. They included an individual apparently referrible to the *Semnopithecus cucullatus*, Isid. Geoff. St.-Hil., although darker in all its markings than is indicated in the description given by the original observer of the species. They also included a species of *Felis*, of a size intermediate between the larger and the smaller animals of that genus, and having in its gray colour and longitudinal striping a general external resemblance to some of the *Viverræ.* This Mr. Bennett regarded as new to science, and proposed to designate it

FELIS VIVERRINUS. *Fel. fulvo cinereus, subtùs albescens; capite, nuchâ, dorso, genis, gulâque nigro vittatis; lateribus, ventre, pedibusque nigro maculatis.*

Long. *corporis* cum capite, 33 unc.; *caudæ* mutilæ, 7; *auriculæ*, 1½.

The prevailing colour of the upper surface is a rather deep yellowish gray, the separate hairs being dusky at the base, yellowish in the middle, and having short black tips. The black lines and spots are formed of hairs destitute of yellow, and having the black tips of much greater length. A longitudinal black band passes on each side, from the inner *canthus* of the eye above the ear nearly to the shoulder; a second, more internally, passes to the same distance backwards, and is somewhat interrupted anteriorly; and between this and its fellow on the *vertex* is the vestige of a median line, which on the forehead is broken up into a double row of spots: these and the two adjoining lines subdivide in front into numerous very small spots between the eyes. Two black lines pass downwards obliquely on either side from below the eye, over the angle of the jaw; and from their terminations on each side there passes a transverse band across the throat: the space between these lines is nearly white, as is also a stripe over each eye, and the whole of the under jaw and chin. There is a large black spot surrounding the base of the ear posteriorly, and the ear is also tipped with black. The long, linear markings of the back are disposed in about five interrupted, longitudinal bands, and some of the spots on the sides assume a linear form.

Of these the most remarkable are, one on each side of the neck, and an oblique wavy band on the shoulder. The spots on the sides generally approach a rounded shape, and form, posteriorly, four or five interrupted longitudinal rows. Those of the under surface are larger, and are arranged without order. On the fore limbs the spots are small externally, and internally there are on each two large transverse black patches. On the hinder limbs the spots are arranged so as to form interrupted transverse bands on both surfaces. The hairs of the soles of the feet are dusky brown. The tail is spotted above in the same manner as the sides; its colour beneath is uniform. The spots are throughout numerous. The whiskers are white, and take their origin from three black lines on either side.

The species is nearly allied to *Felis Serval*, Schreb., but will readily be distinguished by the characters above given, by the comparative shortness and strength of its limbs, and by the locality whence it was obtained.

Specimens were exhibited of three species of *Toucan*, hitherto apparently undescribed, which form part of the Society's Museum. At the request of the Chairman, Mr. Gould pointed out their distinguishing characteristics. He described them as RAMPHASTOS *Swainsonii*, RAMPH. *culminatus*, and PTEROGLOSSUS *hypoglaucus*.

The exhibition was resumed of the new species of *Shells*, forming part of the collection made by Mr. Cuming on the western coast of South America, and among the islands of the South Pacific Ocean. Those exhibited on the present occasion were accompanied by characters by Mr. G. B. Sowerby, which are given in No. VI. of the Society's "Proceedings." Their names were as follows:

TRITON *clathratus*, *nitidulus*, *distortus*, *reticulatus*, *Mediterraneus*, (from the coast of Sicily, but nearly resembles the preceding species, which is from the Gallapagos,) *Ceylonensis*, *lineatus*, and *decollatus*, (the first seven, Mr. Sowerby remarks, may be regarded by some as mere varieties of *Trit. maculosus* of Lamarck, but he is fully satisfied that they are perfectly distinct species;) BULINUS *discrepans*, *calvus*, *ustulatus*, *pallidior*, *Luzonicus*, *conspersus*, *albus*, *striatulus*, *decoloratus*, *unicolor*, *Jacobi*, and *scabiosus*.

Specimens were also exhibited from the same collection, of two species of *Cirripedes*, apparently hitherto undescribed. They were characterized by Mr. Sowerby as POLLICIPES *ruber* and *polymerus*, their characters also being given in the "Proceedings."

Preparations were exhibited of the stomach and *cæcum* of two species of SEMNOPITHECUS, F.Cuv., SEMN *Entellus* and *fascicularis*. They were obtained from individuals which recently died in the Society's Gardens. Mr. Owen called the attention of the Society to these preparations in illustration of a Paper which he read "On the Sacculated Form of the Stomach in the *Monkeys* of the Genus *Semnopithecus*, F. Cuv." He referred to M. Otto as the first observer of this peculiar structure among the *Monkeys*, that eminent anatomist having described and figured it in the "Nova Acta Academiæ Cæsareæ" (tom. xii. p. 511.), as it exists in a species to which he gave the name of *leucoprymnus*, placing it doubtingly among the

Cercopitheci, although it now seems by general consent to be regarded as a *Semnopithecus.* From its existence in M. Otto's species, and in the only two species of *Semnopithecus* which Mr. Owen has had opportunities of dissecting, the latter gentleman is disposed to consider it as appropriated to the genus, which may consequently be now regarded as established on anatomical as well as on zoological and geographical grounds.

Col. Sykes reminded the Society that, in submitting his Catalogue of the *Mammalia* observed in Dukhun, East Indies, he took occasion (Phil. Mag. and Annals, N. S. vol. x. p. 307,) to comment on the popular error respecting the ferocious and untameable disposition of the *common Hyæna, Hyæna vulgaris,* Cuv. His opinions were founded partly on observation of a cub which he had domesticated, and partly on facts communicated by his friends. He went on to state as follows:

"Two years have elapsed since I placed in the Gardens of the Society the above-mentioned cub (a female), which has now attained its full growth, and I am happy to be enabled to confirm the opinions I formerly advanced. In India it was allowed to run about my house, and on board ship it was released from its cage two or three times a day, to play with the sailors and gambol with the dogs. It early recognised my person and voice, and would obey when called; and in general was as playful and good-humoured as a puppy. My visits to it in the Gardens have been rare, and at long intervals, nor have I ever carried it food; I anticipated, therefore, that it would outgrow its early associations, and that I should be to it as any other stranger; but it has always greeted me not only as an acquaintance, but as an old friend; and if I am to judge from its agitation and peculiar cries, the animal's recognition is that of affection.

"On Sunday last it was asleep in its cage when I approached. On calling to it by its name it looked up, distinguished me in the crowd, started on its legs, and on my applying my hand to its mouth to smell to, it threw itself down against the bars, rubbed its head, neck, and back against my hand, and then started on its legs and bounded about its cage, uttering short cries. On ceasing to speak to it, and moving away, it stopped, and looked wistfully after me, nor resumed its motions until I addressed it again. Its manifestations of joy were so unequivocal, as to excite the surprise of a great number of bystanders. As these pleasing traits in the disposition of a calumniated animal appeared so new to those who surrounded me on that occasion, they may possibly be deemed of sufficient interest to be worthy of extended promulgation by record in our Proceedings.

"I take occasion to repeat my conviction, that association with man, constant kindness, and abundance of food, will suffice not only to modify, and indeed eradicate, the worst traits in the disposition of any animal of the higher classes, but give birth to others of which their natures were not deemed susceptible."

June 25.—Extracts were read from a letter addressed to the Secretary by W. Willshire, Esq., Corr. Memb. Z.S., dated Mogadore, May 5, 1833. It referred to various animals of Marocco which Mr. Willshire is in expectation of procuring for the Society. It also stated the opinion of the writer that "the *M'horr Antelope* [recently described by Mr. Bennett as a distinct species, as noticed in Lond. & Edinb. Phil. Mag. vol. ii. p. 477,] will be found to be of the same race as the *Nanguer* of Senegal;" Mr. Willshire "having traced the existence of the *M'horr* to Whadden (or Hoden on the maps), and even further to the southward, thus approaching near to Senegal." Mr. Willshire adds that he considers that "the *Antilope Leucoryx* is almost beyond a doubt the *Bekker-al-wash* of the Arabs of this neighbourhood."

Mr. Willshire forwarded at the same time the following account of the method practised in dressing skins in Marocco, the results of which are excellent as regards the preservation and colour of the fur and the flexibility of the pelt.

"Wash the skin in fresh water to deprive it of the salt; as soon as this is done scrape the flesh off; when take

"2 lbs. alum,

"1 quart buttermilk,

"2 or 3 handfuls barley meal,

"which mix well together, and lay on the fleshy side of the skin equally; fold up and press it together carefully, and let it lie two days. On the third day take it to the sea side, wash the skin well, and when clean and free from the mixture, hang it up to let the water run from it: then take 2lbs. rock [Roche] alum finely powdered, and throw or spread it equally on all parts of the skin; again fold up as before, and allow it to lie three days, when it will be in a proper state to dry in the sun, laid flat without taking away the powder. When it is dry, take a pint or two of fresh water and sprinkle it upon the skin, and again fold it up carefully for about two hours to imbibe the water; then lay it on a table, and after scraping it free from the mixture and flesh, take a sand stone (rather rough) and rub the skin well until it becomes soft and pliable, then hang it in the shade to dry. The process is then complete.

"When the skin is perfect, having the head, horns, &c., take off the horns and fill their cavity with a mixture of equal parts of powdered alum and ashes of charcoal, dissolved in water, and expose them two days to the sun. Saturate the trunks of the horns with 8 ounces of alum dissolved in water, and fold up with the skin, and apply the same on each occasion when employed in curing the skin. The flesh on the head and jaws to be carefully taken off, filling the same with powdered alum. It should remain in the sun until perfectly dry.

"In addition to the foregoing description of the mode used in this country in dressing skins, as related by the person employed by me, it may be well to observe that the process does not take so long here, as I have often received back skins of the *Aoudad* and *Leopard* from the dresser, on the third or fourth, and never exceeding the fifth day, perfectly cured. Allowance has been made by the dresser, in the foregoing description, for the difference in the climate of London.

"The skins of smaller animals must not be subjected to so lengthened a process, or they will become harsh, and the pelt impoverished.—W. W."

A brief description was read of a pair of *Doves*, now living at the Society's Gardens, which had been pointed out by Mr. Vigors as representatives of a species hitherto undescribed. It was characterized as COLUMBA *Princeps*, Vig.

Dr. Grant exhibited a preparation of the *cloaca* of a female *Condor*, *Sarcorhamphus Gryphus*, Dum., which recently died at the Society's Gardens. He entered into a series of observations on the subject, demonstrating the differences of structure and appearance existing in its several parts, and the several orifices opening into it. He adverted to the imperfect development of the right oviduct and ovary in the class of *Birds*, and considered it as probably dependent on the position of the *aorta* in that class. To the position of the *aorta* in the *Mammalia* he was also disposed to attribute the inferior powers of the left side of the animals composing that class, an inferiority which is very striking in the cranial structure of the *Cetacea*, to which he had occasion to refer at the last Meeting of the Society. He dwelt particularly on the *bursa Fabricii*, remarkably evident in this large bird, and explained the several uses which had been attributed to that organ by its discoverer and by subsequent anatomists. With M. Geoffroy-Saint-Hilaire he regarded it as the analogue of Cowper's glands in the *Mammalia*, and adduced various reasons in favour of this view.

Mr. F. D. Bennett exhibited a dried preparation of the upper *larynx* and adjoining parts of the *Albatross*, *Diomedea exulans*, Linn., for the purpose of demonstrating the existence in that bird of an *epiglottis*.

Having demonstrated this and the adjacent parts on the preparation exhibited by him, Mr. F. D. Bennett added that as it had been the opinion of naturalists in all ages that no bird possesses an *epiglottis*, the structure which he had brought under the notice of the Society appeared to him highly interesting. So fixed was the opinion to which he had adverted that when Warren showed the existence in the *Ostrich*, *Struthio Camelus*, Linn., of a structure which he regarded as an *epiglottis*, the denomination was generally rejected even in this anomalous *bird*, and the part was considered as a mere elevation at the base of the tongue, a rudiment, but without the function, of the organ. In the *Albatross*, however, the function is that of an *epiglottis*; and the size, though small, is sufficient for the protection of that portion of the *rima glottidis* which cannot be closed in the manner usual in *Birds* by the apposition of its margins. With a peculiar structure of the *glottis* there exists an apparatus equally peculiar in the class, as a provision against the inconvenience which might otherwise result from the deviation from the normal structure.

Mr. F. D. Bennett also exhibited several specimens of a species of *Pyrosoma* captured by him, on the 6th September 1832, at sea, in lat. 1° 41′ N., long. 11° 56′ W. Between 2 and 4 A.M. the sea, having been two hours before less luminous than usual, presented one mass of bright phosphoric light extending to a considerable distance around the vessel. The extensive field of bright luminous matter emitted so powerful a light as to illuminate the sails, and to permit a book of small print to be read with facility near the windows of the stern cabins. Above this luminous field numerous sea fowl were hovering in search of their prey. The light appeared to be entirely owing to the *Pyrosomata.*

Specimens taken from the sea and placed in a vessel containing sea water, ceased altogether to emit light, or emitted it but sparingly while they remained at rest. On the water, however, being agitated, or when one of the masses of animals was taken into the hand, the whole mass became instantly illuminated by myriads of bright dots, much resembling in hue the points on the *elytra* of a *diamond Beetle, Curculio imperialis,* Fab.

The *Pyrosoma,* thus enveloped throughout its whole extent in a flame of bright phosphorescent light gleaming with its peculiar hue, presented a most splendid spectacle; the light shed by it was sufficient to render objects distinctly visible in every part of an otherwise dark room. If long retained in the hand, or returned to a quiescent state in the water, the luminous spots gradually faded, and no light was visible until the animal was again disturbed, when the illumination instantly returned with all its vivid splendour. After death it emitted no light.

The mass of *Pyrosoma,* of the usual cylindrical form and gelatinous substance, was about 4 inches in length and 1½ in circumference. The tube, passing along its middle, is described as being open at both ends; the orifice at the broader extremity being much better defined in its circular form, larger, and more distinct than that of the opposite end. The surface of the mass appeared to be studded with numerous prominent rigid and pearly tubercles intermingled with small specks of a brown or red colour. In these latter the power of emitting light appeared chiefly to be seated, these being frequently bright while the remainder of the body exhibited only its natural white or yellowish white hue; a hue which changed after death into a red tinge. The brown specks, when removed from the body, did not emit light.

A "Description, with Additional Particulars, of the *Apteryx Australis* of Shaw," by Mr. Yarrell, was read. It described in greater detail than the communication made by the author on February 12, the external structure of this singular bird. It also observed on its probable habits, and on its place in the natural series in immediate relation with the *Struthionidæ.* Following up the history of our acquaintance with it, which commenced with the possession by Dr. Shaw of a single perfect skin (hitherto unique and brought under the observation of the Society by the kindness of the President, of whose collection it now forms part), Mr. Yarrell referred to the incidental notices of it by Captain Cruise, M. Lesson, M. Duperrey, and M. Gaimard, and from the evidence thus collected pointed out its locality to be Mount Ikou-Rangui, near East Cape, New Zealand, and its native name to be *Kiwi*, frequently doubled, according to the custom of the natives, into *Kiwi-Kiwi*. With this information it is hoped that some of our enterprising countrymen in that quarter may, ere long, succeed in acquiring additional specimens and additional knowledge, as regards both the habits and the structure of this curious race.

July 9.—A letter was read, addressed to the Secretary by Charles Telfair, Esq., Corr. Memb. Z.S., and dated Port Louis, February 25, 1833. It gave an account of the history of a gigantic living specimen of the *Indian Tortoise*, *Testudo Indica*, Linn., which has recently been presented to the Society by Lieut. General Sir Charles Colville, late Governor of the Mauritius. The specimen is one of those which were brought from the Seychelles Islands to the Isle of France in 1766, by the Chevalier Marion du Fresne; and is believed to have since remained unchanged in size and appearance. Its length, measured along the curve of the back, is 4 feet 4¼ inches; its breadth, taken in the same manner, 4 feet 9 inches; the length of its *sternum*, 2 feet 8 inches; the breadth of its *sternum*, 2 feet 1¼ inch. Its weight is 285 pounds.

An extract was read from a second letter from Mr. Telfair, of the date of Feb. 26, referring to an animal known in the interior of Madagascar by the name of *Sokinah*. Mr. Telfair regards it as an undescribed species of *Tenrec*, *Centenes*, Ill. A specimen of a very young individual, which was transmitted in spirit by Mr. Telfair, was exhibited, and compared with young specimens of the *European Hedge-hog*, *Erinaceus Europæus*, Linn., and of the *half-spiny Tenrec*, *Centenes semi-spinosus*, Ill. Its extreme youth, however, precluded the possibility of satisfactorily characterizing it. It was born in confinement, and lived for seventeen days; its parents having escaped from their cage on the night of its birth.

A letter was read, addressed to the Secretary by R. J. Bourchier, Esq., Corr. Memb. Z.S., dated Malta, June 8, 1833. It contained an account of two *Vultures*, *Vultur Kolbii*, Daud., (the *Chasse-fiente* of Le Vaillant,) which have recently been presented to the Society's Menagerie by Sir Thomas Reade, Corr. Memb. Z.S., His Majesty's Consul at Tunis. Mr. Bourchier also adverted to his attempts to procure for the Society living *Bustards* from Northern Africa. Although the birds are secured without much difficulty, his attempts have been hitherto unsuccessful, owing to the impossibility of keeping them alive in confinement for any considerable length of time, so inveterately sulky is their nature. He proposes to endeavour to obtain them at a very early age; or, if possible, to procure their eggs and have them hatched under a *domestic Turkey*.

A specimen was exhibited of the Indian variety of the *Nilotic Crocodile*, *Crocodilus vulgaris*, Cuv., obtained in Vellore, and presented to the Society by Alexander Bain, Esq. At the request of the Chairman, Dr. Harlan explained the structure of the heart and the course of the circulation in the *pike-headed Alligator*, *Alligator Mississippensis*, which he had described in detail in the 'Journal of the Academy of Natural Sciences of Philadelphia.'

Specimens of various objects of zoology, collected by George Bennett, Esq., Corr. Memb. Z.S., during his late voyage to New South Wales and in that colony, were exhibited. They were transmitted by Mr. G. Bennett to the Royal College of Surgeons, and the exhibition was made with the permission of the Board of Curators of the College Museum. They included a portion of a *Flying-fish*, to a parasite on which several *Barnacles* (*Cineras*, Leach,) were attached: several *Mollusca*: a river *Lobster*: portions of the *Death Adder*, &c. &c. They also included the *uterus* of a *Kangaroo*, "showing the fœtus with a *placenta* attached, contained within it." Mr. Owen, by whom the preparations were brought under the notice of the Society, and who remarked on each of them as they were severally presented, observed on this that he had not yet examined it sufficiently to determine the structure of the umbilical appendage visible in the preparation. It was accompanied by sketches by Mr. G. Bennett of the fœtal *Kangaroo in utero*, which were exhibited.

The preparations were accompanied by a letter addressed by Mr. G. Bennett to Mr. Owen, and dated Sydney, New South Wales, February 4, 1833, from which several extracts were read. Among them was the following:

"I have a section of one female *Ornithorhynchus* which I shot, in which the milk gland is very large; and I can now inform you from actual observation that milk is secreted from it: it comes out (as your mercury did when you injected the ducts,) in small drops on the surface of the skin. I intend sending you a further account of this; but you can mention it to the Zoological Society as a decided fact; and which had also been seen by some intelligent gentlemen in this country;—but I was not satisfied to assert it until I became an eye-witness of the fact. I wish you to show the specimens to the Zoological Society, with some brief comments in my name, stating also that I am about to send home a detailed account of the habits and œconomy of the *Ornithorhynchus* and *Kangaroo*."

The exhibition was resumed of the new species of *Shells* contained in the collection made by Mr. Cuming on the western coast of South America, and among the Islands of the South Pacific Ocean. Those brought on the present evening under the notice of the Society were accompanied, as on previous occasions, by characters by Mr. Broderip and Mr. G. B. Sowerby. They comprehended the following species of the genus CARDIUM: CARD. *Cumingii, procerum, Orbita, planicostatum, obovale* (remarkable for the peculiarity of its general form; its length and breadth being equal, and its height much greater), *elatum* (*long.* 4, *lat.* 3·5, *alt.* 4·5 *poll.*; being the largest species of the genus with which Mr. Sowerby is acquainted, its dimensions sometimes far exceeding these), *senticosum, multipunctatum, unimaculatum, Consors, laticostatum, maculosum, Panamense, aspersum*, and *multistriatum*.

Dr. Grant communicated the following extract from a letter which he had received from Dr. Coldstream, of Edinburgh:—

"Torquay, (Devon,) Nov. 10, 1832.—Today I examined the *ova* of *Sepia officinalis*. A group of eighteen was attached (each by a ring formed of its semigelatinous coats) to a leaf of *Zostera marina*. They were of an elongated oval shape, about 1 inch in length and $\frac{4}{10}$ths in breadth; colour black, shining; consistence soft. Tunics of the *ovum* very numerous, of various thickness, arranged concentrically. When these tunics were removed in succession until the *ovum* became transparent, I saw distinctly the contained fœtus and its yelk within the inner coat. I could see it move and respire. When the egg was gently pressed, it moved briskly. I succeeded in getting the inner membrane with the contained fœtus out of the egg entire. I kept one in this state in sea-water for many hours, at the end of which time no change had taken place. Others I opened, and let out the fœtus; at first preserving it in its own fluid. Its only evident motion was that of respiration performed with more or less activity, according to the degree of disturbance given to it. When at rest, the respirations were thirty two per minute. The sac was dilated, and the funnel raised as in the adult; and from the transparency of the mantle, I could see plainly the motions of the lateral valves. The surface was marked with several spots; proportionally, not so numerous as in the adult. These seemed to me to become larger after the removal from the egg; but I saw no contraction and dilatation similar to what occurs in the adult. The yelk at first adhered to the front of the body, being placed between the arms; but I could not see how it was attached. In a short time it dropped off. It seemed to consist of a very thin membrane, inclosing a homogeneous transparent jelly. The lateral fin was broad, and, when the animal moved, had much wavy motion. When touched, before the yelk separated, the sac was contracted, raised, and a sharp expiration took place. The same, after separation of the yelk, was sufficient to make the animal move backwards a short distance. When salt-water was mixed with the fluid in which the fœtus floated, the animal, at first, appeared uneasy, drew its mantle over its eyes, and breathed quickly. This agitation, however, soon subsided, and there seemed to be additional vigour imparted. Viewed ventrally, the ink bag's silvery coats were seen shining through the mantle; and when the animal was touched, it twice or thrice ejected minute streams of ink. Whole length of the fœtus $\frac{4}{10}$ths of an inch. The eyes were very large proportionally. The suckers on the arms appeared only as minute tubercles. The shape of the yelk was nearly spherical; diameter about $\frac{2}{10}$ths of an inch.

"Nov. 12.—The fœtus taken out of its egg on the 10th instant was, on the same evening, put into salt water, which happened to be muddy; it continued to respire, and appeared well all the evening; but afterwards its sac contracted so as to allow the lateral valves to be seen outside, and it was languid: next morning it was dead. Today I dissected it. The shell was found loosely imbedded in the mantle. It was $\frac{1}{10}$ths of an inch in length; white; in shape ovate; thickest at the narrow end, where it was almost opaque; composed of five concentric layers; outermost very thin, translucent, spotless; others marked with variously shaped spots; near the margin of the shell these were simple [roundish, oval, or oblong]; towards the centre more complex [elongated and variously but slightly branched]. Internally, I found the gills distinctly, and, to all appearance, perfectly formed. The ink bag contained a considerable quantity of very deep-coloured ink. The inferior pair of arms were very broad at their base, and furnished with a fin-like expansion.

"The fœtus which I laid aside (in salt water), covered with the inner coat only (that membrane being entire), I found this morning outside of it and dead. I opened others of the group of eggs, and found every fœtus dead. Some had ejected part of their ink within the egg. In some the amniotic fluid was, in part, gelatinous. The spots were distinctly visible on the skin of the mantle, head, and arms ; yellowish brown beneath ; darker above."

Mr. Cox read a Paper "On the Circumstances which modify the Existence of Animals in Northern Regions." He dwelt on the migrations of these animals, chiefly in search of food, which in the countries they usually inhabit could scarcely be obtained during the winter months. When the spring returns, and the supply of nutriment becomes abundant, plethora and consequent disease would probably result; but this, the author conceives, is provided against partly by the expenditure of the animal forces for the purposes of generation, and, in the *Ruminants* with deciduous horns at least, by the extra supply of blood required for the renovation of these organs. The horns of the several species of *Deer*, Mr. Cox remarked, appear to be large proportionally with the extent to which the variation in the deficiency and abundance of food at different seasons of the year prevails; those of the extreme north being much more heavy and branched than those of the animals of more temperate regions; and the branching being at its minimum in the *Deer* of India. In still warmer countries and in tropical regions, *Deer* almost cease to exist, their place being occupied by *Antelopes*, *Ruminants* with persistent horns; a provision quite in accordance with the assumed law that the growth of horn is designed to employ superabundant blood produced by excess of nourishment at one period of the year, these animals in which the horns are continually growing having constantly at their disposal food in sufficient and nearly equable quantity.

ROYAL SOCIETY OF EDINBURGH.

"Experimental Researches regarding certain Vibrations which take place between Metallic Masses having different Temperatures." By James D. Forbes, Esq. Professor of Natural Philosophy in the University of Edinburgh.*

The vibrations here referred to are those which, with their accompanying sounds, were first observed by Mr. Arthur Trevelyan, and communicated to this Society in a paper published in the 12th volume of their Transactions. The author of the present paper undertook the inquiry as soon as the remarkable fact was announced by Mr. Trevelyan, and was induced to prosecute it to a considerable extent experimentally, in consequence of being dissatisfied with the only plausible explanation yet offered,—that of the successive expansions of the cold metal by the hot one at the point of contact, at each successive vibration, which was conceived to afford the necessary impulse or maintaining power.

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* The paper of which the above is an abstract, was read before the Royal Society of Edinburgh on the 1st of April, 1833.—Edit.

In this paper the phænomena of sound are first discussed, which, with Mr. Faraday, the author imputes solely to the number of vibrations taking place in a given space of time. This seems completely proved by observation; and the note depends upon the frequency of the oscillations, which have been observed as high as between 700 and 800 in a second, and must often be greatly more frequent. The phænomena of vibration are next considered, as affected by the nature of the metals, by the form of the masses, and by temperature. The order of the metals as vibrators is the following,—meaning that the cold metal must always stand lower in the list than the hot one, and that the force or intensity of vibration is, generally speaking, proportional to the space intervening between two metals on the list:—*silver, copper, gold, zinc, brass, platinum, iron, tin, lead, antimony, bismuth.* Antimony and bismuth are placed at the bottom of the list, because no other metal is capable, under any circumstances which have been examined, of producing vibrations in conjunction with those two metals: they are the only metals yet observed which, when heated, do not vibrate on cold lead.

From experiments detailed at considerable length in the paper, the author is led to the following practical conclusions, which, whatever may be the fate of the hypothesis which he is disposed to found upon them, will, he conceives, be viewed as valuable in themselves:—1. As far as has been observed, the vibrations never take place between substances of the same nature. 2. Both substances must be metallic. 3. The vibrations take place with an intensity proportional, within certain limits, to the difference of the conducting powers of the metals for *heat* (or *electricity*), the metal having the least conducting power being necessarily the *coldest.* 4. The time of contact of two points of the metals (between which the oscillations take place) must be longer than that of the intermediate portions. 5. The impulse is received by a distinct and separate process at each contact of the bar with the block, and in no case is the connexion of these points in any way essential. 6. The intensity of vibration is (with certain exceptions) proportional to the difference of temperature of the two metals.

From these data the author first endeavours to show that the hypothesis of expansion is untenable, by tracing closely the process of communication of heat, and proving that it must lead to several conclusions totally at variance with experiment, and particularly that, as far as conducting power for heat is concerned, both the hot and the cold metal should possess it in the highest degree. The author is led by the striking analogy of the powerful repulsive action of electricity in passing from a good to a bad conductor, to infer a similar property in heat, which, without entering into any speculations as to the nature of those principles, appear to have a repulsive character in common, indicated by a tendency to diffusion and equilibrium. He conceives, that while some very delicate experiments in France have given indications of the actual force exerted by heat equally diffused through two adjoining masses, the energy in this case is produced by the accumulated repulsive power in the last particles of the good conductor, the current (without meaning anything hypothetical by the term,) being suddenly cut short by the resistance opposed to its passage by the inferior conductor. The destructive energy of electricity indicative of its repulsive force is never exerted in a state of equilibrium, but by the accumulation of separate repulsive energies which takes place in the transition from a good to a bad conductor, or during its passage through the latter.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Twentieth Annual Report of the Council.

"The Fourth Volume of Transactions, reported at the last Meeting as being in the press, has since been published; and the Council have great pleasure in stating that it has attracted considerable attention, and has been most favourably noticed by several distinguished geologists. Its leading feature, Dr. Boase's paper on the Geology of the County generally, adduces many facts at variance with the prevailing theories, and advances some peculiar notions, which have given rise to much discussion, and awakened a novel interest in the investigations of Primary Geology. The Council, therefore, earnestly hope that Dr. Boase will be induced to prosecute, in all its details, a subject, which forms so important a part of the objects of this Society. The Volume also contains much additional and curious information on the recent deposits of Cornwall.

"Mr. Henwood's progress in the survey of the mines has been retarded by the duties of an office in the Duchy, obtained for him by the application of the Society to their patron, His Most Gracious Majesty. He has, however, already examined the principal mining districts, and made a variety of experiments on the electro-magnetism of veins, and on the intensity of terrestrial magnetism: their results have brought to light some additional facts, by the accumulating of which alone, we may expect to arrive at any satisfactory conclusions on these obscure but interesting phænomena.

"Several donations of mineralogical and geological specimens have been received by the Society during the past year, which will be particularized in the Curator's Report. Since Mr. Carne's list of the minerals found in St. Just was published in our Second Volume, a considerable number have been discovered, not before known to exist there, of which he has now given a descriptive account: and the Council rejoice that he is still labouring in that field which he has hitherto so successfully cultivated.

"These numerous contributions have so nearly filled the cabinets, that it cannot be long before some additional accommodation will be required, which may be obtained by the enlargement of the present rooms, without incurring any considerable expense.

"It is impossible for a cursory observer to form an idea of the extent of the geological collection; for several series, and among them Mr. Henwood's specimens of veins and veinstones, exceeding a thousand in number, are deposited in drawers.

"Since the last Meeting, Dr. Boase's numerous and instructive specimens of Cornish rocks have been labelled and arranged: and the Council have the satisfaction to state, that measures are about to be taken to place the classification of the remaining geological series on the same respectable footing as that on which the mineralogical collection stands, through the able and zealous exertions of Mr. Giddy, who, they are sorry to announce, has, through indisposition, been under the necessity of tendering his resignation as Secretary, but has kindly consented to continue his valuable services as Curator."

(By Order) E. C. GIDDY,
August 30th, 1833. Secretary.

The following papers have been read since the last Report:—1. On the Progress and Prospects of the Society. By Henry S. Boase, M.D. &c.—2. Additional Contributions to the Mineralogy of the Parish of St. Just. By Joseph Carne, Esq. F.R.S. F.G.S. M.R.I.A. &c. Treasurer of the Society.—3. Report of further Progress made in the Geological Survey of the Mines of Cornwall. By W. J. Henwood, Deputy Assay Master of the Duchy of Cornwall, F.G.S. London and Paris, Hon. M.Y.P.S. &c. Member of the Society.—4. Preliminary Observations on the Coast of the Land's-end District. By the Rev. George Pigott, Member of the Society.—5. An Attempt to elucidate the Nature of the Primary Strata, and more particularly that of Quartz Rock. By H. S. Boase, M.D.—6. On the Occurrence of the Earthy Phosphate of Iron in a Metalliferous Vein. By Joseph Carne, Esq. &c.—7. Observations on the Intensity of Terrestrial Magnetism, on Carn-Brea Castle, on the surface of Dolcoath Mine, and at 210 fathoms deep in the same Mine. By W. J. Henwood, F.G.S. &c.—8. Notice of a Granitic Elvan in the Granite at Pedn-merer-mere, near St. Levan. By the Rev. George Pigott.—9. On the Nature and Origin of the indigenous and transported Deposits of Cornwall, belonging to the Modern Geological Epoch. By H. S. Boase, M.D.—10. On some curious Intersection of Veins in Dolcoath Mine. By W. J. Henwood, F.G.S. &c.—11. An Account of the Quantity of Tin produced in Cornwall and Devon, in the year ending with Midsummer Quarter 1833. By Joseph Carne, Esq. &c.—12. An Account of the Quantity of Copper produced in Cornwall, and in Great Britain and Ireland, in the year ending the 30th June, 1833. By Alfred Jenkin, Esq.

Curator's Report: Donations to the Museum and Library.—Eighty-five specimens of Fossil Plants from the Shale of the Jarrow Colliery, near Newcastle. Presented by the Natural History Society of Newcastle-on-Tyne.—Specimens of Sussex Marble and Malm-rock; and also of a Copper-lode in Poldice Mine. By Davies Gilbert, Esq. &c. &c.—Numerous specimens of Minerals, Rocks, and Organic Remains, from various localities. By J. Mitchell, LL.D. Esq. &c., London.—Specimens of Graham Island in the Mediterranean. Presented by John Davy, M.D. F.R.S. &c. &c.—A Fossil Fish, referred to by Professor Sedgwick in his paper on the Magnesian Limestone of the northern part of England. By George T. Fox, Esq., Durham.—A fine specimen of Gray Copper-ore from Wheal Perran Mine. By R. S. Scott, Esq.—A curious pseudomorphous formation of Carbonate of Iron, and of Iron Pyrites, from Virtuous Lady Mine, near Tavistock. By Edward Pearse, Esq.—Another specimen of the same. By Mr. William Longmaid.—Specimens illustrative of the Stream-work near Marazion Bridge; and four hundred specimens of Cornish Rocks. By W. J. Henwood, F.G.S. &c.—One hundred and fifty additional specimens of the Cornish Rocks. By H. S. Boase, M.D.—Specimens of Woodstone from Hobart Town, Van Dieman's Land; and Silver and Copper-ore from South America. By Edward Leah, Esq.

At the Anniversary Meeting, held on the 30th August, 1833, Davies Gilbert, Esq. F.R.S. &c., *President*, in the chair;—the Report of the Council being read, it was resolved, That it be printed and circulated among the Members;—That the Council be empowered to nominate Students, who, under certain regulations, may be permitted to enjoy gratuitously the benefits of the Library and Museum; and that the management of this department be entirely entrusted to the Council;—That the thanks of the Society be presented to the authors of the various papers, to the officers, &c. The President then announced that the Council had come to the determination of recommending to the Society to present a piece of plate to Dr. Boase for his valuable services during the last four years; and he was sure that the proposal would be carried by acclamation: it was therefore resolved, That a piece of plate be presented to Dr. Boase.

The following gentlemen were elected Members of the Society:—*Honorary Members:* Roderick Impey Murchison, Esq. F.R.S. &c. &c.; Henry T. De la Beche, Esq. F.R.S. &c. &c.; Charles Lyell, Esq. F.R.S. &c. Professor of Geology, King's Coll. London.—*Ordinary Members:* John J. A. Boase, Esq.; Mr. Charles Parry; the Rev. Thomas Pascoe; Edward H. Rodd, Esq.; Mr. Richard Rule; the Rev. Thomas H. Vyvyan.—*Associate:* Mr. William Petherick, of Dolcoath.

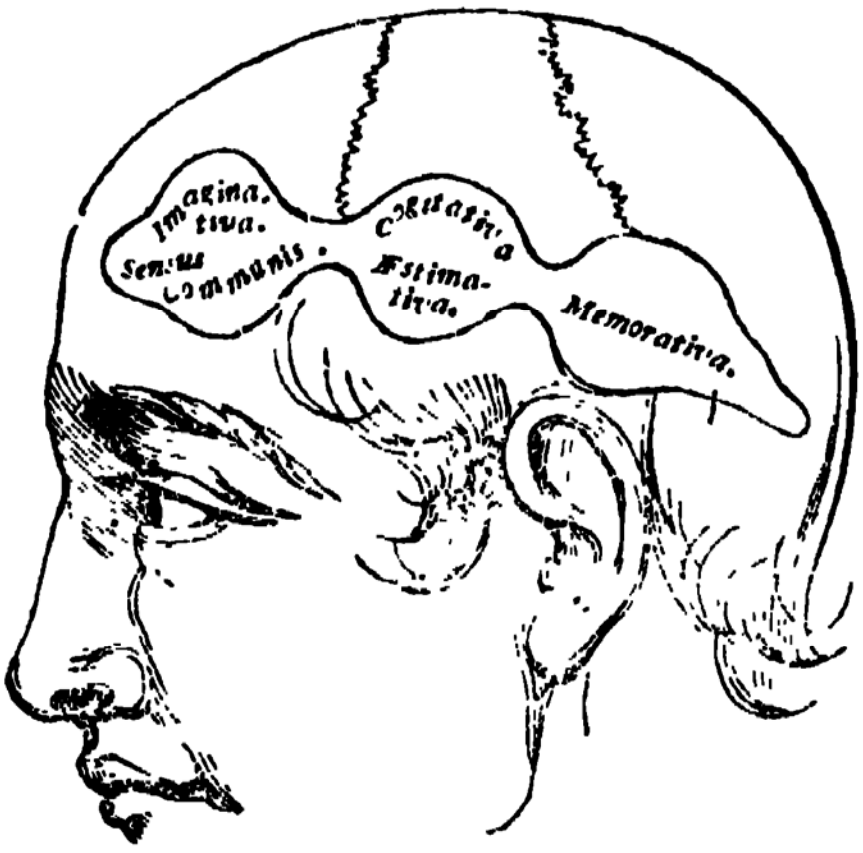
Officers and Council for the present year:—*President:* Davies Gilbert, Esq. F.R.S. &c. &c.—*Vice-Presidents:* George S. Borlase, Esq. F.R.S. &c.; John Paynter, Esq.; Sir Charles Lemon, Bart. M.P. F.R.S. &c.; John Hearle Tremayne, Esq.—*Secretary:* Henry S. Boase, M.D.—*Treasurer:* Joseph Carne, Esq.—*Librarian:* the Rev. George Pigott.—*Curator:* Edward C. Giddy, Esq.—*Assistant-Curator:* W. J. Henwood, F.G.S. &c.—*Council:* J. J. A. Boase, Esq.; T. Bolitho, Esq.; Wm. Cornish, Esq.; Stephen Davey, Esq.; Robert W. Fox, Esq.; Day P. Le Grice, Esq.; George D. John, Esq.; Rev. Canon Rogers; Wm. M. Tweedy, Esq.; John Williams, jun., Esq.

The Quarterly Meetings of the Society for the ensuing year will be held on Wednesdays,—the 23rd October, the 15th January, the 16th April, and the 23rd July.

LIII. *Intelligence and Miscellaneous Articles.*

EARLY ANTICIPATION OF PHRENOLOGY.

THE Rev. W. D. Conybeare, F.R.S. &c., has favoured us with the following notice of a curious anticipation of the modern Phrenological System, bearing date as early as 1503: it occurs in an old Encyclopædical kind of Work, entitled *Margarita Philosophica*, printed at Friburg in that year. The author speaking of the mental functions, says, "Sensus interiores numero quinque sunt: Sensus Communis, Imaginativa, Æstimativa, Cogitativa, et Memorativa. *Horum Organa in substantiâ cerebri* subtilissimis secernuntur pelliculis; quæ primum totum cerebrum tribus distinguunt ventriculis, quorum anterior et medius rursus bipartiuntur:—1ma portio anterioris organum est Sensus Communis; 2da, Imaginativa. 1ma, autem ventriculi medii attribuitur Æstimativæ; 2da, Cogitativæ; posterior vero ventriculus totus Memorativæ deputatur." This is illustrated by the sketch of a head divided just like one of Gall or Spurzheim's models, a part of which is copied in the annexed.


Imagina. tiva.
Sensus communis.
Cogitativa
Æstima- tiva.
Memorativa.

COMPOSITION OF PHOSPHURETTED HYDROGEN.

M. Rose published some years since researches on the composition of phosphuretted hydrogen gas, which, he observes, do not agree with those published at the same time by Dumas and by Buff: he has since repeated his experiments, both upon the spontaneously inflammable gas, and that obtained from hydrated phosphorous acid.

Spontaneously inflammable Gas.—M. Rose does not state the composition of this gas by weight; but states that he considers it as composed of 1 atom of phosphorus and 3 atoms of hydrogen, or, by calculation, of $\frac{1}{2}$ a volume of vapour of phosphorus $+ 1\frac{1}{2}$ volume of hydrogen gas, condensed to 1 volume. Now as neither the absolute weights of the constituents of the gas, nor those of the atoms are given, we are compelled to make them out indirectly. M. Rose finds that hydriodic acid combines with phosphuretted hydrogen, atom to atom, and 100 parts are stated to consist of 78·73 acid + 21·27 phosphuretted hydrogen. Assuming the atomic weight of the acid to be 127, that of phosphuretted hydrogen will, of course, be 34·30 (78·73 : 21·27 : : 127 : 34·30). Subtracting then 3 = 3 atoms of hydrogen from 34·30, we have phosphuretted hydrogen composed of

3 atoms of hydrogen	3·
1 atom of phosphorus	31·30
Weight of atom	34·30

Now whether we admit 15·7 as the atomic weight of phosphorus, as stated by Dr. Turner, or 16, as given by Dr. Thomson, we may consider phosphuretted hydrogen as composed of

3 atoms of hydrogen	3·	or	3·
2 atoms of phosphorus	32·		31·4
	35·		34·4

It is therefore a sesquihydruret.

M. Rose procured the gas by heating hypophosphite of lime: the gas contains variable proportions of hydrogen: the volume of the gas obtained was accurately measured, and remained for thirty-six hours exposed to a weak solution of sulphate of copper, deprived by long boiling of atmospheric air: the phosphuretted hydrogen gas was completely absorbed, and the hydrogen only left. M. Rose states that the specific gravity of phosphuretted hydrogen by calculation should be 1·1846; that which was procured from phosphite of lead was of sp. gr. 1·205.

M. Rose proved by several experiments, that the spontaneously inflammable phosphuretted hydrogen gas, does not lose this property, when kept over either mercury or water. He states also that it is requisite to dry the gas perfectly by means of chloride of calcium, as without this precaution it may contain vapour of phosphorus, which would increase the weight of the gas; and he thinks that is the only way in which the different sp. gr. found by himself and M. Dumas can be explained.

Phosphuretted Hydrogen from Phosphorous Acid.—This gas was obtained by heating the hydrated phosphorous acid in green glass retorts: white ones must not be used, as the acid readily attacks them: a phosphite is formed with the alkali of the glass, which, by the action of heat, becomes a phosphate, and disengages hydrogen gas. The gas received was dried over chloride of calcium, and the latter portions of it rejected. The gas obtained is a mixture of varying proportions of hydrogen and phosphuretted hydrogen. Much depends upon the degree of heat employed: when it was quick and intense, the gas was much richer in phosphorus.

M. Rose found the action of the inflammable and uninflammable gases to be perfectly similar on different substances; and they have both the property of being converted, one into uninflammable and the other into inflammable phosphuretted hydrogen: on these accounts M. Rose considers them as isomeric compounds.—*Ann. de Chim. et de Phys.* tom. li. p. 1.

On referring to Dr. Dalton's New System of Chemistry, vol. ii. p. 182, it will appear that he had several years since arrived at the same conclusion as M. Rose, with respect to the similarity of the two kinds of phosphuretted hydrogen.

"It may be proper," he observes, "to advert more particularly to the hydrophosphoric gas of Davy. That this gas is the same as that we have been describing, can hardly admit of a doubt. Their near agreement in sp. gr., in their absorbability by water, in the quantity of oxygen requisite for their combustion, in their moderate expansion by burning with a minimum of oxygen, and in their combustibility in oxymuriatic acid, are circumstances sufficient to warrant their identity."

Dr. Dalton states the sp. gr. of the spontaneously inflammable gas to be nearly 1·1, which much more nearly approximates to 1·1846, Rose's calculated statement, than the sp. gr. given by Dumas, viz. 1·761. It may be further observed that Dr. Dalton's analysis does not widely differ from that of Rose: he states the gases to be composed of 1 hydrogen + 9 phosphorus, which will give

Hydrogen	3·5
Phosphorus	31·5
	35·0

R. P.

METALLIC PHOSPHURETS.

M. Rose obtained phosphuret of copper by passing a current of hydrogen gas over heated phosphate of copper. This salt became at first yellow by the conversion of the peroxide into protoxide, and afterwards, at a higher temperature, it became gray phosphuret of copper, water being formed at the same time. It consisted of

Copper . . .	65·09	or nearly	1 atom of copper . . .	64
Phosphorus .	34·91		2 atoms of phosphorus	32
	100			96

Phosphuret of Cobalt.—A current of hydrogen gas, directed upon phosphate of cobalt, gave very pure phosphuret: water only was given out. This compound is gray, powdery, and gives no phosphoric flame with the blowpipe.

When exposed to a current of dry chlorine, and moderately heated, strong ignition occurred; chloride of phosphorus sublimed, and chloride of cobalt crystallized. Its composition was found to be

Cobalt . . .	73·47	or nearly	3 atoms of cobalt . .	90
Phosphorus	26·53		2 atoms of phosphorus	32
	100·			122

Phosphuret of Nickel.—This phosphuret may be obtained, as well as those of copper and cobalt, by decomposing the muriate with phosphuretted hydrogen gas. It is black and insoluble in muriatic acid; but dissolved by nitric acid, it burns under the blowpipe with a phosphoric flame. Phosphuret of nickel may also be obtained from the phosphate by hydrogen gas.

Phosphuret of Iron.—This was procured by the action of phosphuretted hydrogen gas upon sulphuret of iron, at a gentle heat.

It is powdery, insoluble either in concentrated or dilute muriatic acid, but dissolved by aqua regia and nitric acid. It burns when heated by the blowpipe with a phosphoric flame.

Phosphuret of Chromium.—The decomposition of anhydrous crystallized chloride of chromium is not effected by phosphuretted hydrogen below a red heat, which occasions the gas to deposit phosphorus. The phosphuret of chromium preserves the form of the chloride: it is black, insoluble in muriatic acid, and very slightly dissolved by nitric acid and aqua regia: it burns with the blowpipe, giving a phosphoric flame. It is composed of

Chromium . . .	64·5	or 1 atom of chromium . .	29
Phosphorus . .	35·5	1 atom of phosphorus . .	16
	100·		45

In attempting to convert other metallic chlorides and sulphurets into phosphurets by means of phosphuretted hydrogen, satisfactory results were not obtained. Though at first combined with phosphorus, it was lost by continuing the heat necessary for the decomposition of the chlorides or sulphurets.

Phosphuretted hydrogen gas very readily decomposes chloride of silver, but metallic silver only is obtained; chloride of lead is not so quickly decomposed, but the results are similar. Muriatic acid gas is disengaged, and phosphorus is deposited in the cool parts of the apparatus. Chloride of mercury, decomposed by the gas, produces a phosphuret. There is a very violent disengagement of muriatic acid gas, but the combination is destroyed by heat.

Chloride of zinc may be converted into phosphuret, but the quantity obtained was too small for examination. Chloride of manganese also gives a phosphuret by the action of phosphuretted hydrogen: it has a metallic lustre, but does not give a phosphoric flame with the blowpipe. Protosulphuret of tin is very slowly decomposed at a low temperature by phosphuretted hydrogen gas; sulphuretted hydrogen gas is disengaged and phosphorus sublimes. No phosphorus was discoverable in the residue, but it still contained sulphur: it dissolved entirely in muriatic acid with disengagement of sulphuretted hydrogen gas.

Sulphuret of bismuth is also reduced by the action of phosphuretted hydrogen gas to the metallic state, and so also is sulphuret of antimony; phosphorus is deposited; sulphuretted hydrogen is given out: it is, however, remarkable that the greater part of the antimony sublimes, although the temperature is not raised to the degree at which the metal volatilizes *per se.*—*Ann. de Chim. et de Phys.* tom. li. p. 47.

ON ARICINA, SANTALINE, SARCOCOLINE, &C. BY M. PELLETIER.

Aricina.—This is an organized salifiable and crystallizable base, accidentally discovered by MM. Pelletier and Coriol in examining some bark, which had the characters of yellow bark, but which it was stated did not yield any quina. Aricina, when combined with sulphuric acid, forms a compound which is more soluble in hot than in cold water, and gelatinizes on cooling, provided the solution is perfectly neutral when examined by litmus paper: if, however, excess of acid be added, another sulphate is formed which crystallizes in flat needles; cinchonia, on the other hand, crystallizes with sulphuric acid in neutral solutions.

The following is the composition of this substance:

By direct Analysis.		Atomic Constitution.	Calculated Results.
Carbon	71·00	20	70·93
Hydrogen	7·00	24	6·95
Azote	8·	2	8·21
Oxygen	14·	3	13·96

Estimating the atomic constitution according to the weights generally adopted in England, the atoms of hydrogen and azote in this statement, and the following analyses, will be reduced to one half the above numbers. M. Pelletier remarks, that if the analysis of aricina be compared with those of cinchonia and quina by Liebig, it will be seen that they may be represented as consisting of a common radical, united with 1, 2, 3 atoms of oxygen; this radical being $C^{20} H^{24} Az^2$, we shall have

$$(C^{20} H^{24} Az^2) + O = \text{Cinchonia.}$$
$$(C^{20} H^{24} Az^2) + O^2 = \text{Quina.}$$
$$(C^{20} H^{24} Az^2) + O^3 = \text{Aricina.}$$

M. Pelletier is of opinion, however, that cinchonia contains two atoms more of hydrogen than were found by M. Liebig. M. Pelletier observes that cinchonia, quina and aricina will then be considered as three degrees of oxidizement of the same substance, which explains the fact that aricina requires more acid to saturate it than the other two; and it will explain how two salifiable bases, as he discovered with respect to quina and cinchonia, may exist in the same bark.

Santaline.—This is the colouring matter of red saunders wood (*Plerocampus santalinus*). An account of this substance has been read before the Academy of Sciences, to which the author refers; but he mentions a curious circumstance respecting it. Sulphuric æther does not immediately dissolve santaline; the process takes place slowly, and the solution, instead of being red, as in alcohol, is orange or even yellow. By the spontaneous evaporation of the æther, exposed to the air, the colouring matter is obtained of a superb red; if the æther be quickly evaporated *in vacuo*, the colour is much less intense, often it is even quite yellow. It is further remarked, that deprived of water, as the æther employed may be, and although the santaline may have been perfectly dry, water always remains after the evaporation of the æthereal tincture: it sometimes even happens that ice is obtained when the æther is rapidly evaporated under the receiver of the air-pump. M. Pelletier seems inclined to believe that while dissolving in the æther, the santaline loses a portion of its oxygen, which forms water with the hydrogen of the æther, and that afterwards, the santaline, by exposure to the air, regains its colour by absorbing oxygen.

M. Pelletier states, that some chemists consider santaline as a resinous matter; he is however more inclined to rank it with acid colouring matters, on account of its affinity for salifiable bases.

Its composition is

	By direct Analysis.	Atomic Constitution.	Calculated Results.
Carbon	75·03	16	75·36
Hydrogen	6·37	16	6·15
Oxygen	18·60	3	18·48

Sarcocoline.—This substance was discovered by Dr. Thomson in sarcocol, the concrete juice of the *Pœnea mucronata.* It is obtained by treating sarcocol with sulphuric æther to remove the resinous matter; the sarcocol is then to be dissolved in absolute alcohol, which is to evaporate spontaneously.

Sarcocoline is soluble in water, and more so when it is hot; the solution made in boiling water becomes milky on cooling; it is soluble in alcohol, insoluble in æther, and does not crystallize under any circumstance. When treated with nitric acid, it is converted into oxalic acid. The results of the analyses are

	By direct Analysis.	Atomic Constitution.	Calculated Results.
Carbon	57·15	13	57·39
Hydrogen	8·34	23	7·94
Oxygen	34·5	6	34·65

Piperine.—Piperine is a peculiar crystalline substance found in several species of the fruits of the genus *Piper.* It was discovered by Œrsted. In order to obtain piperine, the method proposed by M. Pelletier or M. Poutet, may be employed; the latter process gives the piperine more free from fatty matter.

The composition is

	By direct Analysis.	Atomic Constitution.	Calculated Results.
Carbon	70·41	20	70·54
Hydrogen	6·80	24	6·91
Azote	4·50	1	4·08
Oxygen	18·28	4	18·45

It contains one atom more of oxygen and one less of azote than aricina. [To be continued.]

PREPARATION OF FORMIC ACID.

The following is the method of preparing formic acid, adopted by M. Döbereiner.

"I dissolve one part of sugar in two parts of water, mix the solution in a copper alembic, with $2\frac{1}{2}$ to 3 parts of peroxide of manganese, well powdered; I heat the mixture to about 140° Fahr. and carefully stirring with a wooden rod, I add 3 parts of sulphuric acid, previously mixed with an equal weight of water. On the addition of the first third of the diluted acid, so strong an effervescence is produced, that the mixture would overflow the vessel, if it were not fifteen times larger than required to hold the mixture; along with carbonic acid, very pungent vapours of formic acid are given out; the head of the alembic must then be put on and communication made with the refrigeratory to condense the vapours. As soon as the action of the acid is over, the remaining two thirds of the acid are put into the boiler; the mixture is to be well agitated, and distillation carried on almost to dryness, in order that all the formic acid produced may pass into the receiver. A limpid liquid acid is obtained which has a penetrating odour; it is composed of water, formic acid, and an æthereal matter. It is to be neutralized with a carbonate, (chalk answers best,) and evaporated; this last operation ought to be performed in a retort with a receiver adapted to it: if it be wished to separate the æthereal matter, which evaporates with the water and remains dissolved in it, it may be effected by distilling the aqueous product from chloride of calcium. A pound of sugar furnishes formic acid enough to saturate 5 or 6 ounces of carbonate of lime. The residue is protosulphate of manganese, artificial malic acid, and a kind of extractive matter. The sulphate of manganese may be employed in dyeing instead of sulphate of iron, to deoxidize indigo. If concentrated formic acid or formic æther be required, the formic acid produced from the sugar is to be saturated with carbonate of soda; the solution is to be evaporated to dryness, and 7 parts of the dry salt, reduced to powder, are to be distilled either with 10 parts of sulphuric acid and 4 of water, or with a mixture of 10 parts of concentrated sulphuric acid, and 6 parts of perfectly rectified spirits of wine. The formic æther produced in the latter case ought to be agitated, if it is acid, with a little calcined magnesia; it may be separated from alcohol by agitating it with a little water, and it is deprived of the water by distilling it with chloride of calcium.

"The properties of this æther are well known; one of the most remarkable is, that when put in contact with water, the bodies composing it are reproduced, namely, alcohol and formic acid. When it is mixed with dilute alcohol, it suffers no alteration; hence a mixture of 1 part of formic æther and 3 parts of rectified spirit may be preserved, (*spiritus formico-æthereus*)."—*Ann. de Chim. et de Phys.* tom. lii. p. 105.

EXPERIMENTS ON THE ACTION OF LIME ON CERTAIN SOLUTIONS OF CARBONATE OF POTASH. BY HENRY HOUGH WATSON.

Among the Miscellaneous Articles in the London and Edinburgh Philosophical Magazine and Journal of Science for September 1832, will be found an extract from *Ann. de Chim. et de Phys.* xlix. p. 142, in which M. Liebig states that "if one part of carbonate of potash be dissolved in four parts of water, and the solution be boiled with slaked lime, the potash does not lose the smallest quantity of carbonic acid; it does not become caustic, even though lime be added to any extent, or however long the boiling may be continued." Now, as the first part of this statement is considerably at variance with the one which I should give, as founded on the result of my own inquiries, without further premise I venture to lay before the readers of the Philosophical Magazine a few experiments lately made on the same subject.

Exp. 1. 50 grains of pure dry carbonate of potash, prepared by exposing the bi-carbonate to a red heat, are dissolved in 200 grains of water (4 times the weight of the carbonate); and to the solution are added 70 grains of proto-hydrate of lime. The mixture is heated to boiling as quickly as possible, the whole heating operation only lasting two minutes. The liquor is filtered while hot and undiluted, and a part of it treated with sulphuric acid, sp. gr. 1·135, of which 60 grain measures are required before effervescence takes place, and only 20 more for saturation; consequently $\frac{3}{4}$ths of the carbonate are decomposed by the lime.

The boiling point of the mixture is about 220°; but I find that a very material decomposition of the carbonate takes place even when a like mixture is exposed only to 150° for a quarter of an hour, being briskly agitated all the time.

Exp. 2. 61·4 grains of pure dry carbonate of potash = 1 atom, are dissolved in 320 grains of water = 40 atoms, or about $5\frac{1}{5}$ times the weight of the carbonate; and to the solution are added 90 grains of hydrate of lime: 50 grains more water are also added to the mixture as an allowance for the loss in boiling. It is now heated up to boiling in $1\frac{1}{2}$ minute, and kept boiling another $1\frac{1}{2}$ minute, when it is found by weighing that the additional 50 grains of water have evaporated.

The liquor is filtered while hot and undiluted, and part of it treated with sulphuric acid, such as before, of which 100 grain measures are required before effervescence takes place, and only 5 more for saturation. In this case, therefore, the lime has only left $\frac{1}{21}$ part of the carbonate undecomposed.

Exp. 3. 61·4 grains of pure dry carbonate of potash = 1 atom, are dissolved in 480 grains of water = nearly 8 times the weight of the carbonate, and to the solution are added 90 grains of hydrate of lime: 50 grains more water are also again added to the mixture. It is boiled as before, until the additional 50 grains of water are evaporated.

The liquor is filtered while hot and undiluted, and a part of it found to require 105 grain measures of the sulphuric acid for saturation; only a few minute bubbles of carbonic acid being given out.

It therefore appears that to obtain caustic potash, not fewer than about 53 atoms of water (besides what is combined with the lime to constitute the hydrate,) are sufficient for each atom of the carbonate.

The atomic weights here made use of are those of Dr. Dalton.

How M. Liebig arrives at the conclusion that carbonate of potash loses no carbonic acid when dissolved in only 4 times its weight of water, and boiled with slaked lime, I am not aware, except it is from the entertainment of some theoretical views. He explains the matter upon the fact that concentrated potash takes carbonic acid from lime. Though that may be the case, it is very evident, from the result of my first experiment, that the solution employed is not sufficiently strong to do so.

Considerable time has now elapsed since the statement in question was announced; but as nothing contradictory has, as far as I know, hitherto been given, the above experiments and observations will not, I trust, be regarded as too late, when it is considered that the real intent of their publication is the correction of error.

Little Bolton, July 13th 1833.

PROPOSED MODIFICATION OF THE PATENT LAWS.

The following paper has been privately circulated; and at the present time, when the attention of many persons is directed to the law of patents, it may prove interesting to some of our readers.

Proposed Definition of the Subject Matter of Patents for Inventions.—That His Majesty is hereby empowered to grant letters patent, of exclusive privilege, for limited periods of time, as hereinafter provided, to the inventor or inventors, or to the introducer from abroad, of any manufacture, which, at the time of lodging application for such letters patent, shall be new in the United Kingdom of Great Britain and Ireland; which new manufacture shall consist, either in a new saleable thing made, or in some new means or process, employed in the working, making, or preparing of a saleable thing: and such letters patent shall confer on such inventor or inventors, or on such introducer from abroad, and their executors, administrators, and assigns, the exclusive privilege of making and selling such new manufacture, so far as the same shall consist in a new saleable thing made, and the exclusive privilege of using and practising such new manufacture, so far as the same shall consist in some new means or process, employed in the working, making, or preparing of a saleable thing; always provided, that no such privilege shall interfere with any privilege conferred by previously granted letters patent.

That any manufacture shall be accounted new at the time of application being lodged for letters patent for the same, when the said manufacture has not been openly made or sold, nor openly used or practised, during the whole, or any part of the *fifteen* years immediately preceding the lodging of such application; although such manufacture has been openly made or sold, or openly used or practised, prior to the said *fifteen* years, or although such manufacture has been secretly made or sold, or secretly used or practised, during some part of the said *fifteen* years; always provided, that the said open making or selling, or open using or practising prior to the said *fifteen* years, or the said secret making or selling, or secret using or practising during some part of the said *fifteen* years, has been unknown to the person making application for letters patent, at the time of lodging such application, and that the said secret making or selling, or secret using or practising has been abandoned before lodging such application. But no manufacture shall be accounted new in the United Kingdom of Great Britain and Ireland, at the time of application for letters patent for the same being lodged, when the said manufacture has been described, either in some printed publication made in the said United Kingdom, during the *thirty* years immediately preceding the lodging of such application, or in any specification of the letters patent for the whole, or for any part or parts of the said United Kingdom, granted during the said *thirty* years, but expired, or made void; always provided, that in such publication, or in such specification, the said manufacture has been so described, as to enable manufacturers, or intelligent operatives employed by them, to make, or to use and practise, the said manufacture.

That any manufacture, which may otherwise be accounted new, according to the foregoing description, shall be so accounted, although the novelty shall consist only in a new combination of previously known parts, or only in a new improvement; but, in such cases, the exclusive privilege shall be confined to the new combination, or to the new improvement.

DEATH OF THOMAS ALLAN, ESQ., OF LAURISTON, F.R.S. L. & E. F.L.S. &c. &c.

The friends of Mr. Allan will receive with deep regret, and those who saw him so lately at the Cambridge Meeting of the British Association, not without surprise, the intelligence of his death; for though evidently declining in health, yet there was nothing then to indicate that the termination of his earthly career was so near at hand. After returning to Edinburgh, he again left that city early in September, attended by some members of his family; and when on a visit to his friend Mr. Bigge, of Linden Hall, Northumberland, was suddenly attacked with apoplexy, and died there, on the 12th ult., in the 57th year of his age.

The claim of Mr. Allan to be honourably remembered by the cultivators of science, rests mainly on his extensive and accurate knowledge of mineralogy, and on his contributions to its advancement. He was thoroughly acquainted with the system of external characters, and most skilful in applying them to the discovery of those minute resemblances and differences on which the classification of minerals is founded. Instances of his skill must be in the memory of those who intimately knew him. It may be sufficient to notice his sagacious discrimination from Gadolinite, of the mineral to which the name of *Allanite* was afterwards given by the eminent chemist who analysed it*. The collection formed by Mr. Allan, though by no means the largest, was undoubtedly the best-selected and the most instructive in the kingdom. It was also admirably arranged, partly by himself and partly by Mr. Haidinger; and the latter derived, for his Work explanatory of the System of Mohs, many of his descriptions and figures from the fine specimens in Mr. Allan's cabinet. These treasures Mr. Allan delighted to lay open, not for the gratification of vanity (with which no man was ever more entirely unstained), but for the higher purposes of communicating knowledge, and of receiving information in return.

Mr. Allan was one of those who early espoused the Huttonian Theory of the Earth. He was struck with the beauty of that happy generalization, and warmly admired the creative genius of which it was the offspring. But he did not adopt it till he had himself tried its conformity with actual phænomena; and even then he admitted that it was by no means free from difficulties. In the course of this scrutiny, several new observations occurred to him, which are detailed in a series of memoirs published in the Transactions of the Royal Society of Edinburgh. His only separate Work is an octavo volume on Mineralogical Nomenclature, which passed through two editions, and which it would now be desirable to reprint, with the additions rendered necessary by the present state of the science.

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* Dr. Thomson.

The mind of Mr. Allan was one of considerable original power and of great and general activity. "In every sense of the word" (as it has been expressed by one who knew him well,) "he was an eminent citizen of Edinburgh." During the last twenty years, his name has been associated with every scheme for the improvement of that beautiful city; and on this subject he was completely an enthusiast, devoting a large portion of his time and thoughts to the public good. Of the various Institutions of Edinburgh, whether for charitable or other purposes, there was scarcely one to which he had not rendered important services; and those, more particularly, intended for the promotion of science, were the objects of his constant and fostering care. At the meeting of the British Association in Edinburgh during the ensuing year, to which he looked forward with great delight, the loss of his zealous cooperation and of his kind attentions and friendly offices will be felt as a serious calamity.

The following is a list (probably incomplete) of Mr. Allan's publications:—1. "On the Rocks in the Vicinity of Edinburgh:" Transactions of the Royal Society of Edinburgh, vol. vi.—2. "Remarks on the Transition Rocks of Werner:" Ditto, vol. vii.—3. "An Account of the Mineralogy of the Faroe Islands:" Ditto.—4. "Description of a Vegetable Impression found in the Quarry of Craigleith:" Ditto.—5. "Observations on Chalk Strata, and on the Construction of Belemnites."—6. "On a Mass of Native Iron from the Desert of Atacama in Peru."—7. "Remarks on a Mineral from Greenland, supposed to be Crystallized Gadolinite."—8. "An Account of the Geology of the Environs of Nice."—A few Articles in Dr. Brewster's Encyclopædia. W. H.

DESCRIPTION OF A NEW AND INTERESTING OBJECT FOR THE MICROSCOPE. BY C. GOULD.

Having occasion to prepare some of the scales from different fish for the microscope, in viewing one as an opake object, in a drop of water, I observed a very curious phænomenon; which was, a number of minute shining particles in motion, moving in all directions, appearing and disappearing, and when on the surface reflecting the most brilliant prismatic colours, producing a beautiful microscopic effect. I have examined them with various forms of the microscope, particularly under a fine achromatic belonging to Mr. Joinville; and they have every appearance of animalcula. Several of my microscopic friends who have seen them are of the same opinion, particularly the above gentleman, who has paid great attention to microscopic pursuits. Whether they are living animalcula or not, I am at a loss to determine, and must leave it for the investigation of our microscopic friends. The readiest way of examining them is as follows:—Take a single scale from any fish (say a fresh herring); put it on a flat piece of glass, and cover it with a drop of pure water; press the scale to make it lie flat on the glass; place it under the microscope with a piece of black paper, or the black ivory stage-piece of the microscope, as it must be viewed as a perfect opake object, or the effect is not seen: by means of a condenser throw a strong light upon it from a lamp or candle; and you will observe a multitude of minute bodies in motion on the scale: but they are much more visible in that part of the drop of water which the scale does not occupy, forming a most beautiful and interesting microscopic object.

LUNAR OCCULTATIONS FOR OCTOBER AND NOVEMBER.

Occultations of fixed Stars by the Moon, visible at Greenwich in the Year 1833. *Computed by* THOMAS MACLEAR, *Esq.; and circulated by the Astronomical Society.*

⁂ The *angles* are reckoned *from* the northernmost point, and also *from* the vertex, *towards* the right hand, round the circumference of the Moon's image, as exhibited in an inverting telescope.

An Asterisk (*) annexed to the time of the phænomenon is intended to denote that the Star is *on*, or *near to*, the meridian, at that time.

1833.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean time.	Angle from		Sidereal time.	Mean time.	Angle from	
						North Pole.	Vertex.			North Pole.	Vertex.
				h m	h m	°	°	h m	h m	°	°
Oct. 7	33 Cancri	6	1037	2 2	12 56	140	99	2 36	13 31	214	172
	38 *o* Cancri	7	1048	5 34	16 27			a near	approa		
	39 Cancri	6	1050	5 5	15 59	72	38	6 16	17 9	272	247
	40 Cancri	6	1051	5 9	16 3	63	28	6 18	17 11	279	254
	(136) Cancri	7	1058	6 3	16 56	53	27	7 9	18 2	285	274
13	88 Virginis	7	1571	17 16	3 48	109	137	18 1	4 33	201	235
18	24 Sagittar.	6·7	2141	20 23	6 36	25	39	20 55	7 7	337	359
22	29 *x* Aquarii	6	2613	21*25	7 21	54	47	22 8	8 4	352	355
28	(155) Arietis	7	284	1 18	10 50	114	111	2 37*	12 9	304	314
	85 Ceti	6	286	1 52	11 24	141	135	3 6*	12 37	277	285
31	123 ζ Tauri	3·4	684	11 53	21 11	17	57	12 7	21 25	347	26
Nov. 1	(338) Tauri	6·7	759	22 44	8 1			a near	approa.		

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, and Mr. Veall at Boston.

Days of Month, 1833.	Barometer.					Thermometer.					Wind.			Rain.			Remarks.
	London.		Penzance.		Boston	London.		Penzance.		Bost. 8½ A.M.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.	
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.								
Aug. 1	30·394	30·379	30·292	30·286	29·83	67	45	71	56	61	N.	NE.	N.	...	...	...	*London.* — August 1. Cloudy and cool. 2, 3. Overcast. 4. Very dry. 5. Cloudy and fine. 6. Cold and dry: fine. 7—9. Fine. 10. Fine: lightning and rain at night. 11—14. Fine. 15. Heavy dew: fine. 16, 17. Fine. 18. Overcast. 19—21. Fine. 22. Fine: strong gale at night. 23. Dry and windy. 24—27. Fine. 28. Very dry. 29. Slight haze: fine. 30. Fine: heavy rain with strong wind at night. 31. Very boisterous with heavy rain. On the afternoon of the 4th, about 4 P.M., a whirlwind swept through the garden from north to south: four large sashes were lifted by it and dashed to pieces; one of them was tossed over a low wall to the distance of twenty yards. *Penzance.*—August 1. Clear: fair. 2. Clear. 3. Clear: fair. 4—12. Clear. 13. Fair. 14. Clear. 15. Clear: rain at night. 16. Fair. 17, 18. Clear: light shower. 19, 20. Fair. 21, 22. Fair: evening rain. 23. Fair. 24. Fair: rain at night. 25. Fair. 26, 27. Clear. 28, 29. Fair. 30. Clear: heavy rain. 31. Fair: a shower. *Boston.*—August 1. Cloudy. 2. Cloudy: rain early A.M. 3. Cloudy. 4. Fine. 5. Cloudy. 6, 7. Fine. 8. Cloudy. 9, 10. Fine. 11. Cloudy: rain early A.M. 12. Cloudy. 13. Cloudy: rain A.M. 14. Cloudy. 15. Foggy. 16. Cloudy: rain A.M. and P.M. 17. Fine. 18. Cloudy. 19. Fine. 20. Fine: rain A.M. 21. Cloudy: rain P.M. 22, 23. Stormy. 24. Fine. 25. Cloudy: rain early A.M. 26—28. Fine. 29. Fine: rain P.M. 30. Fine. 31. Rain and stormy: blew a hurricane all day; ships wrecked. orchards stript, and trees blown up by the roots.
2	30·372	30·326	30·298	30·292	29·83	65	52	67	55	58·5	N.	NE.	calm	...	...	0·05	
3	30·349	30·292	30·298	30·278	29·75	67	45	67	53	60	N.	E.	NE.	...	...	...	
4	30·351	30·223	30·304	30·298	29·82	74	45	67	55	60	NE.	NE.	calm	...	...	...	
5	30·111	30·100	30·204	30·098	29·50	70	43	68	51	62	NW.	E.	N.	...	...	...	
6	30·153	30·135	30·104	30·098	29·14	70	36	67	53	57	NE.	E.	calm	...	...	...	
7	30·134	30·077	30·084	30·048	29·54	75	45	67	50	58	NE.	NE.	calm	...	...	...	
☾ 8	30·076	30·058	30·054	30·018	29·48	76	51	68	51	61·5	NE.	NW.	calm	...	...	...	
9	30·055	30·006	30·105	30·004	29·44	79	44	68	53	60	SW.	NW.	calm	...	...	...	
10	30 017	29·978	30·001	29·998	29·39	78	44	69	52	62·5	W.	NW.	NW.	0·04	...	...	
11	30·117	29·985	29·998	29·998	29·39	78	37	68	52	58	W.	NE.	calm	...	...	·03	
12	30·188	30·037	30·054	30·048	29·67	74	48	68	53	57	NE.	NE.	calm	...	...	...	
13	29·839	29·669	29·901	29·698	29·25	70	48	64	52	58·5	S.	NE.	calm	...	...	...	
14	29·679	29·639	29·698	29·654	29·16	70	40	63	51	57	NE.	NE.	NW.	...	...	·12	
● 15	29 859	29·757	29·804	29·757	29·27	70	46	64	48	55	N.	NE.	calm	...	0·100	...	
16	29·905	29·791	29·904	29·804	29·27	69	40	65	51	54·5	NW.	N.	calm	...	...	...	
17	29·913	29·843	29·904	29·904	29·36	75	44	64	52	59	W.	N.	W.	...	...	·25	
18	29·822	29·773	29·854	29·851	29·21	74	56	67	52	58	W.	NW.	calm	·11	...	...	
19	29.929	29·742	29·904	29·854	29·17	72	48	68	53	62·5	W.	NW.	W.	...	...	·05	
20	29·979	29·915	30·018	30·004	29·24	74	60	67	55	61	W.	NW.	NW.	...	...	...	
21	29·891	29·691	29·998	29·898	29·24	77	48	67	58	61	W.	SW.	W.	...	·050	·04	
☽ 22	29·771	29 679	29·884	29·858	29·10	71	48	66	53	58	W.	W.	W.	...	·095	·06	
23	29·803	29·654	29·898	29·804	28 94	71	47	65	53	59	W.	NW.	W.	...	...	...	
24	30·011	29 948	29·998	29·954	29·33	74	47	64	54	58	SW.	NW.	NW.	...	·100	·15	
25	30·279	30·127	30·148	30·004	29·59	65	62	65	55	57	SW.	NE.	calm	...	...	·07	
26	30·305	30·235	30·207	30·198	29·67	75	40	65	50	58	NE.	NE.	calm	...	...	...	
27	30·244	30·178	30·204	30·198	29·60	78	45	67	48	62	W.	W.	NW.	...	...	...	
28	30·244	30·198	30·204	30·198	29·59	80	44	67	52	62	W.	NW.	calm	...	...	...	
29	30·158	29·927	30·151	30·048	29·43	80	51	67	52	64·5	SW.	NW.	W.	...	...	...	
○ 30	29 893	29·266	29·951	29·354	29·21	70	45	65	55	58	W.	SW.	W.	·86	·790	·16	
31	29·333	28·985	29·704	29·460	28 25	55	46	60	47	50	NW.	NW.	N.	·92	...	1·04	
	30·394	28·985	30·304	29·354	29·37	80	36	71	47	58·9				1·93	1·135	2·02	

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LIV. *On the Vibration of heated Metals. By* ARTHUR TREVELYAN, *Esq.; including a Letter on the same Subject by Dr.* W. KNIGHT.*

AS it is always interesting to know the accidental circumstances which lead to the discovery of any new fact in natural science, I shall begin by relating the manner in which I was led to observe the vibrations of heated metals. On the 9th of February 1829, I was about to spread with a plaster-iron some common pitch, when the iron being at too high a temperature, I placed it in a slanting direction against a block of lead, the handle resting on the table: shortly after doing so I heard a shrill high tone, which much resembled a note produced on the chanter of the smaller Northumberland pipes, an instrument played on by my father's gamekeeper. Not knowing the cause of the sound, I thought he might be practising out of doors. The room being on the ground floor, I went outside, when the sound was no longer heard; and seeing no person, returned again to the room, when I was surprised at still hearing the sound as shrill as before. After looking about for a few minutes, I at last approached closer to the iron, perceived the vibration, and thence found the immediate cause of the sound: I was much surprised and pleased; but at that time, not having studied any branch of natural philosophy connected with the subject of heat, I was not aware that it was a fact which had not been before observed (although it struck me so at the time). I came to Edinburgh in November 1830, and attended Dr. D. B. Reid's class of

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* Communicated by the Author. Part of this paper was read before the Section of Physics of the British Association, meeting at Cambridge, June 27, 1833.

Practical Chemistry. Having mentioned the circumstance and shown him the experiments, he informed me that the fact was new, and recommended me to make further experiments, and prepare a paper on the subject for the Royal Society of Edinburgh. I took his advice, and during the winter session had the honour of having two papers read before that Society, which were illustrated by experiments.

Since that time I have made numerous additional experiments, and have succeeded in obtaining vibration with most metals, many of which I failed with previously, from the apparatus not being sufficiently delicate.

On the opposite page is a Table of the different metals in which I have observed vibration.

The experiments were repeated several times before I was satisfied with the results.

I have not yet observed any effect with bismuth, although I have with pale solder, which is an alloy of that metal.

The gold was in too small a piece to try cold, with the hot bar placed on it. The results with it and with platinum would probably have been more numerous, if I could have obtained better-shaped pieces of those metals.

Bars of tin, lead, bismuth, antimony, block tin, solder, tin-solder and pale solder, produced no effect, when heated and placed on cool blocks, or rings, of all the different metals mentioned in the Table.

Cold bars of lead, bell-metal, tin-solder and pale solder, when placed on heated iron or brass, produced vibration and tone. The cold lead block, placed on the heated polished bar of a fire-grate, sounds loudly, and vibrates rapidly.

The vibration continues in the exhausted receiver of an air-pump.

The bars begin to vibrate on lead at a temperature below 212° Fahr., but on harder metals they require a higher temperature.

A cool brass bar, 5 inches long, 2 inches wide and $\frac{3}{8}$ths of an inch thick, placed on a cool lead cylinder $\frac{1}{2}$ an inch thick, 2 inches in height and $4\frac{1}{4}$ inches in diameter, with a spirit-lamp placed under the bar, produced vibration and tone in 6 minutes 15 seconds, and continued sounding for 5 hours and 55 minutes: when I removed the lamp, it ceased vibrating in 6 minutes, but might probably be continued, by the continued application of the lamp, for any length of time. The block had arrived at so high a degree of temperature, that it was too hot to hold with the naked hand: the vibration was only kept up by the brass bar being so superior to lead in its power of conducting caloric. From the small size of the bar, it soon fell to the same temperature as the lead, and then of course the vibration ceased.

Bars.	Simple Metals.										Alloys.							
	1. Platinum.	2. Gold.	3. Silver.	4. Copper	5. Iron.	6. Cast Iron.	7. Zinc.	8. Tin.	9. Antimony.	10. Lead.	1. Fine Brass.	2. Com. Brass.	3. Gun Metal.	4. Bell Metal.	5. Cock Metal.	6. Block Tin Solder.	7. Tin Solder.	8. Pale Solder or Fusible Metal.
Platinum							+	+		+				+	+	+		
Gold										+	+	+		+	+			
Silver............	+				+	+	+	+	+	+	+	+	+	+	+	+	+	+
Copper..........	+		+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Iron.............							+	+	+	+				+		+	+	+
Cast Iron								+		+								
Zinc								+	+	+				+			+	+
Fine Brass......				+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Common Brass							+	+		+		+	+	+	+	+	+	
Gun Metal......								+		+				+		+	+	
Bell Metal......										+							+	
Cock Metal.....										+				+				

The crosses denote that vibration took place when the heated bar was placed on the cold metal named at the top of the column. The names of metals at the side of the Table are those of the bars which were heated and placed on the cold metal.

When the brass bar at a high temperature is placed on the cool lead, the vibration seldom fails taking place instantly and spontaneously.

A bar 5 inches long, 2 inches wide, and $\frac{3}{8}$ths of an inch thick, vibrated when supporting a weight of 12 pounds.

However near the bar and block may be brought to each other, no apparent phænomenon takes place until they are in actual contact.

By balancing the bar in a horizontal position on a narrow lead block (fig. 1. p. 332), rounded on the part on which the bar rests, the vibration is well exhibited, the bar moving vertically and laterally at the same time. A rod 10 or more inches long, flattened in the centre to prevent its slipping, with a ball on each end (fig. 5), when placed across a heated vibrating bar (fig. 6), increases the arc of motion, rendering the movements much more conspicuous.

A thick ring of copper, 5 inches in diameter, when heated, and hung on a lead bar, vibrates backwards and forwards; and when laid on a narrow lead block, upwards and downwards.

The heated bar vibrated on a piece of thin sheet lead, either placed loosely or soldered on brass, and on a lead block burnished with gold-leaf.

A heated copper bar vibrated on the bottom of a glass tumbler. This is an experiment which the late Professor Leslie tried with me, but it is very uncertain and difficult to obtain.

The bars vibrate best when placed on blocks of lead with the surface somewhat rough: both metals also should be kept clean, and free from oxidation, which impairs the vibration.

A bar of heated copper was placed on an iron block rounded on the surface, and being nicely balanced on the centre of the rounded part, showed the vertical motion.

The shape or size of the bars and blocks is of little consequence, except for the more delicate experiments with the hard metals: on lead the hard metal almost of any form will vibrate when heated.

Mr. K. T. Kemp, the skilful electrician and chemist, informed me that after casting some bismuth, whilst it was cooling, after being taken out of the mould, he heard a sound proceed from it; when cool he again heated it, but could not reproduce the sound.

The bars I use to show the experiments are of different sizes: a bar about 5 inches long, 1$\frac{1}{4}$ inch wide, and $\frac{3}{8}$ths of an inch thick, will produce considerable tone; a rod of small wire 6 inches long, must be attached at one end, to serve as a handle. A ridge is formed along the centre of one side of the broad part, by its being bevelled off towards each edge; the other side is hollowed out down the centre so as to throw the weight on each side, which assists the vibration: the longitudinal ridge is the part that rests on the block, (figs. 3 & 4.).

A hollow lead cylinder about ½ an inch thick, with the diameter in proportion to the length of the bar, is the best form for showing the vibrations and producing the tone.

An inequality or unequal notch on the lead cylinder increases the sound. When the bar at a high temperature is first placed on the cold lead, the tone is very harsh and undefined; but when the lead has acquired a certain proportion of heat, it becomes clear, full and mellow.

Pressure on the bar alters the note: the greater the pressure the higher is the note. Pressure also applied to the sounding-board or table on which the vibrating bar may happen to be placed, or even walking across the room, by altering the position of the bar, changes the tone.

A common poker heated and placed on a lead block vibrates, producing deep tones. If the vibrating bar be placed on a piano-forte, and certain notes be struck on the instrument, the vibration of the bar, and consequently the tone produced by it, is altered, and sometimes suddenly stopped.

Vibration is prevented by rubbing the surface of the lead with mercury, oil, plaster of Paris, or by oil gilding; also if a piece of thin tissue paper, or of wire-gauze, be interposed between the bar and the block.

The vibrations have not been produced with any substances besides metals, except in the instance of the glass tumbler already mentioned.

To try whether there was any attraction between the hard and soft metals when heated, I had two pieces of brass, about the size of an ounce weight, with a wire attached to them, and then hung on a delicate scale beam; having heated the brass weights, blocks of lead were placed under them and tried at various distances, but without producing the slightest effect.

To try whether electricity caused the phænomenon or made any alteration whilst the bar was vibrating, a hole was drilled in a bar and filled with mercury, a wire was soldered to the lead cylinder on which it was placed, and a galvanic battery of 150 plates, 4 inches square, being charged, the wire from the cylinder was placed in one end of the battery, whilst from the other end a wire was brought to the mercury contained in the cavity of the bar, which was heated, and in motion, previous to completing the circle. We also tried other methods, but with none of them did we succeed in producing any change either in the tone or in the vibration.

A Leyden phial was charged with electricity, and applied to the same bar and block, without producing any effect.

Spirit of wine, or æther, evaporated off the heated bar when in motion, produced no effect.

Air blown with a pair of bellows on the heated bar when vibrating does not affect the tremors.

If the heated bar be ground smooth, on the resting part, and the block of lead be also very smooth, no vibration takes place.

When the bar and block become of the same temperature, the vibration and sound cease.

A cylinder, 5 inches long, with a thick side bevelled off, leaving a flat ridge in the centre, similar to the former bars, when heated and placed on lead, produced no change of tone.

From the above-related experiments it appears:

1st, That in order to produce the vibrations, metals must be employed, either of the same kind or different, for both the bars and blocks; with the single exception of the glass tumbler.

2nd, That the difference of temperature between the two metals must be considerable, although some require a much higher temperature than others: the vibrations on zinc and bell-metal succeed with a lower temperature than others of the hard metals.

3rd, That the surface of the block must have some degree of unevenness, for when rendered quite smooth, the vibration does not take place; but the bar cannot be too smooth.

4th, That the interposition of any matter prevents vibration, with the exception of a burnish of gold-leaf, the thickness of which cannot amount to the 200,000dth part of an inch.

5th, That the air has no share in the production of the vibratory movements, however much its presence is essential to the production of sound.

6th, That it has no connexion with galvanism or electricity, the vibration and tone not being affected by passing a current through the bar when in action. Professor Forbes found no appearance of thermo-electricity after trying a number of experiments.

7th, That all the metals, both simple and alloys, produce vibration and sound, when one is heated and the other cool, on being brought into contact, (with the exception of bismuth,) but with some we find much greater difficulty in obtaining vibration than in others, as it depends on the temperature, but more particularly on the mode of placing the one on the other, which with some requires great nicety.

8th, Although all the metals are not found to vibrate on each other, or on metals of the same kind,—and I have not been able produce vibration with other substances,—yet I do not despair but that hereafter, when this subject is better understood, we shall be able to produce vibration with all the metals, and with all other matter either solid or liquid, when heat is applied to a cool substance, or *vice versâ.*

The following theory, which is partly derived from the late Professor Leslie's mode of accounting for the vibration, and appears the most probable, (and experiments hitherto made strengthen this view,) ascribes the vibratory movements to the usual mechanical changes which caloric occasions in passing from one substance into another,—I mean the expansion and contraction which accompany alternations of temperature.

It appears that some degree of roughness of one metal is essential to the success of the operation. This slight asperity arises from numberless points or ridges projecting from the mass of metal.

When the heated bar is laid upon the cool lead, the caloric passes into these prominences; and as their power of conduction is not great, it does not rapidly diffuse itself through the rest of the mass: of course they instantly expand and elongate, and by that sudden elongation they give an impulse to the incumbent bar. Soon, however, the caloric moves into the adjoining mass, and the prominences contract, and at the same time come into a state ready to admit a renewed accession of caloric from the bar: they receive that caloric, again expand, and give a second impulse to the bar. This goes on incessantly, and though the first impulse be infinitely small, and altogether inadequate to produce any sensible movement of the bar, yet by incessant repetition an accumulation of effect takes place, and the movements gradually reach a sufficient magnitude to become easily discernible.

As soon as the bar and block arrive within a certain limit of difference of temperature, the impulses become feebler and feebler, and at length the bar comes to rest. It has been mentioned that the smoother the bar is so much greater is the effect: I conceive that this smoothness operates by increasing the celerity with which the surface possessed of it communicates the caloric to the projecting points of the block, and thereby the elongation which gives the impulse to the bar is increased both in quickness and extent.

It is obvious that had the bar any considerable degree of asperity, the points of contact between the two metals would be fewer, and the passage of caloric between them more tardy.

When the two surfaces are highly polished, the experiment does not succeed: no tremors occur. This result proceeds probably from the circumstance, that the caloric enters into every part of the surface of the block equally, and is more quickly diffused through the mass, and hence there are none of those partial and sudden expansions which give the tremor-causing impulse.

The rocking on the narrow lead block may be induced by two causes, either by some slight inequality in the weight of the portions of the bar on the two sides of the ridge, or some difference in the condition of the surface of that part of the block which the ridge of the bar touches; and may be thus explained:

1st, If the first-mentioned inequality exist, as soon as the bar receives an upward heave, the greater weight of the one side will cause it to incline to that side; and as soon as that heave ceases, and the contraction succeeds, the bar approaches its original position, but will not remain in it, for the inclination given to the preponderating side will, on its return, of course, cause the bar to incline to the opposite side. The impulses which the bar receives, in this position, from the renewed expansion and elongation, will not only renew the upward heave, but also incline it again to the preponderating side, and thus increase the lateral movement, which, like the vertical, though altogether insensible at first, by incessant frequent repetition accumulates and increases till the rocking becomes conspicuous, and is rendered much more so by the transverse rod.

2ndly, If there exist any difference in regard to asperity in the condition of the surface of that part of the block upon which the ridge of the bar rests, it must necessarily follow, that the impulse given to the bar on that side which is most rough will be greater than on the other, and consequently the upward heave will be so modified as to create an inclination to one side. The bar thus thrown off its balance to the right on ascending, will incline as far to the left on descending, and there receiving the expansive impulse, it will be driven back, and thus the principle of rocking will be created. The sound depends upon the rapidity of the vibrations; for when slow, no tone is heard.

I think from the above-related experiments, and the conclusions and theory drawn from them, that the hitherto unknown causes of many sounds are now accounted for.

The sounds described by Humboldt as heard at sunrise by those who sleep on certain granite rocks on the banks of the Orinoco, also the sounds at sunrise produced by the statue of Memnon, and the twang, like the breaking of a string, heard by the French naturalists, as if proceeding from a mountain at Carnac, are probably caused by the pyrometric expansions and contractions of the heterogeneous material of which the statue and the mountain consist. Similar sounds, and from the same cause, are emitted, when heat is applied to any connected mass of machinery, and the snapping heard in a fire-grate affords a familiar example. We have often heard of a poker producing tone when heated and the point rested on a knot in a fir board in the floor of a room; also the singing in a tea-kettle is another example. In distilleries, shortly after the fire is put to the cool coppers, a very loud note is given out, and continues until the liquor boils*.

John Robison, Esq., Secretary to the Royal Society of Edinburgh, informed me that he once let a heated bar fall from his hand: it alighted on a painted shelf of wood, when he was surprised at hearing sounds; but they soon ceased.

The following theory of the cause of earthquakes and volcanic eruptions strikes me as being not at all unlikely. Earthquakes, and the sounds accompanying them, may be caused by vibration, occasioned by heat generated far below the surface of the earth, in some enormous metallic mass, which being in contact with some cool substance, not a very good conductor of heat, the latter is violently agitated, thus producing the vibration felt in earthquakes. By its intensity chasms are opened on the face of the earth; and below, caverns filled with condensed combustible matter and liquid lava are torn open, and the contents, by their enormous expansion, and having found an egress more easily upwards, rise to a great height above the surface of the earth.

The following interesting remarks on the vibration of heated metals are copied from a letter received by my brother from Dr. W. Knight, Professor of Natural Philosophy in Marischal College, Aberdeen, dated June 8, 1833.

"I regret that I have not written sooner in reply to your letter of the 9th ult., but I waited until I should find a convenient opportunity for repeating many of the experiments which I hinted the general nature of to you in my letter of the 19th of April. These opportunities have unfortunately not occurred yet, from myself being engaged with a daily class here, and my family at a country residence at some distance; nor are they likely to occur soon, so that I must content myself with sending you a shorter account than I could wish of the new trials made on the vibrations of metallic bodies in the end of March and beginning of April last.

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* The sounds from the statue of Memnon and the mountain at Carnac, and those from machinery and a fire-grate, have already been explained, nearly in the above words, by Sir John Herschel, (see Lond. and Edinb. Phil. Mag. vol. i. p. 221,) but without reference, however, to the new phænomena of vibrating metals, with the cause of which, indeed, contrary to Mr. Trevelyan's opinion, we apprehend that they are not essentially connected. The sounds heard on the granite rocks of the Orinoco are attributed by Sir J. Herschel, in the same letter, to sonorous vibrations of the air passing through small orifices, either subterranean or communicating with the atmosphere. The singing in a tea-kettle, and the note given out by stills, are referrible, we conceive, to another order of causes, connected with the rapid condensation of vapour, and quite distinct from all the former as well as from those concerned in the vibration of heated metals.—Ed.

"Having frequently failed as well as succeeded with the iron pokers and masses of lead, &c., and not being always able to account for the failures, a simple plan of producing vibrations occurred to me, which in no instance yet fails upon the greater number of metals employed. This consists in placing a quantity of a melted metal (as tin, lead, fusible metal, bismuth, &c.) in a hemispherical, or, better, in a parabolic conoidal cup of copper, or iron, or brass, lying above a piece of lead or other metal. The oscillations of the cup thus resting upon a small surface continue long after it has become solid, indeed until the cup and its contents have fallen to a temperature not much above that of the metal upon which they rest. I have employed in many trials cups of iron, brass and copper, of various sizes, from 2 to 6 inches in diameter at the top; and of melted metals, from an ounce to some pounds, of bismuth, tin, lead, zinc, &c. (In case of pouring the melted metal too rapidly from the ladle into the cups, and thus overturning the latter, it may be steadied by a small pair of forceps for a few seconds.) No vibrations could be produced *above* any other bodies than metals: they are most striking *above* lead, tin, zinc, antimony; more feeble above silver, gold, platina, brass, copper. Above wrought and cast-iron, I could not observe them. They are very distinct above slabs of the metal of reflecting telescopes, and above fusible metal. The smoothness of the ingots above which the vibrations are performed lessens the effect greatly, or prevents it.

"Among the most interesting trials are:

"1st, 3 or 4 ounces of lead, melted and poured into the parabolic cup of copper, resting above the ingot of 1 pound of tin with uneven surface. The tremors loud, quick and large; *a peculiar crackling noise* is heard, which I cannot distinguish in character from the noise of the same kind which ensues on bending a piece of block tin held close to the ear. On compressing the tin ingot with the fingers, the sound does not cease, but assumes a sort of stifled character: on removing the fingers, the former crackling noise is resumed. The fingers feel distinctly the vibration communicated to the ingot by the heated cup.*

"2nd, The same placed on an ingot of 1 pound of zinc. The vibrations commenced immediately on pouring out the lead: they were small, rapid and equal, (not of an unequal sort like those above antimony): the sounds louder than above any other metal. They cease on pressing the ingot between the fingers; when resumed, there seemed some difference of tone: the stopping at the end sudden and distinct.

"3rd, The vibrations of the cups above a platina coin were very slow, and did not commence until after the lead had become solid: they soon ceased;—as if connected with the bad conducting power of platina?

"4th, The solidifying of the melted metals in the cup, and their crystallization when made to solidify during their vibrations, offered several singular phænomena, particularly with bismuth, lead and tin. As far as trials have yet gone, the crystallizing property seemed to be more conspicuous when solidifying during the vibrations, than when the same quantities of metals were not vibrating.

"These observations, like those of your brother, seem to connect this subject closely with the arcana of cohesion. On seeing the copy of your brother's paper, which you were so good as to send me, on the 19th of April, I was naturally much interested in the perusal, and cannot but think his theory the true one. An extensive field seems to be opened for examining the passage of heat through metallic bodies, in connexion with the structure of their atoms in cohesion. I could wish much that he or you would repeat some of the above experiments. I do not know when I shall find time to resume them. I found indeed, soon after beginning, that I could not advance without one of the small metallic Breguet's thermometers, with which the temperature of the ingots might be approximated to. I omitted to mention that the cups themselves will vibrate, if heated above a gas flame; but the other mode of pouring from a ladle some melted metal is more convenient, and increases the range of the phænomena; or hot mercury may be poured in, but the fumes are disagreeable.

"P.S. If you try any of the experiments, it will give me great pleasure to hear of your success. I have tried them so very often that I am quite sure you will not lose your trouble."

At one of the meetings of the Royal Society of Edinburgh this spring, Professor Forbes, of the Edinburgh University, read an able paper, and one that showed great research, on the vibration of metals; he also produced tables showing the difference of metals in the conduction of heat and electricity, and for showing the vibrations: there was great similarity in the position of the different metals in the three columns of the table, showing the difference of power in producing the three various effects*.—In the Literary Gazette for May 17, 1831, Mr. Faraday is mentioned to have shown the experiment of a curved silver plate vibrating and sounding when placed on cold iron, and to have stated that this effect had long been known to working silversmiths.

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* An abstract of Prof. Forbes's paper will be found in our last Number, p. 303.—Edit.

Description of the Figures.

Fig. 1. Is the lead block on the rounded surface of which the bar rests, by that means the vibrations being more distinctly seen: this block is screwed to a flat brass plate, having three small flat knobs to rest on.

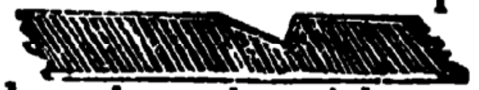
Fig. 2. Shows the ring on which the bar is to be placed to produce the tone: it is better to have an unequal notch for the bar to rest on, thus 

Fig. 3. Is the back part of the bar, showing the ridge on which it ought to rest when placed on the lead.

Fig. 4. Is the upper side of the bar, hollowed out in the centre so as to bring the gravity more to each side.

Fig. 5. Is the bar with a ball at each end, to be laid across the vibrating bar, so as to cause it to move through a larger arc.

Fig. 6. Shows how the bars ought to be arranged for exhibiting both the vertical and lateral motion.

Fig. 7. Shows the bar as placed for producing the tone.

Figs. 3. 4. 5.

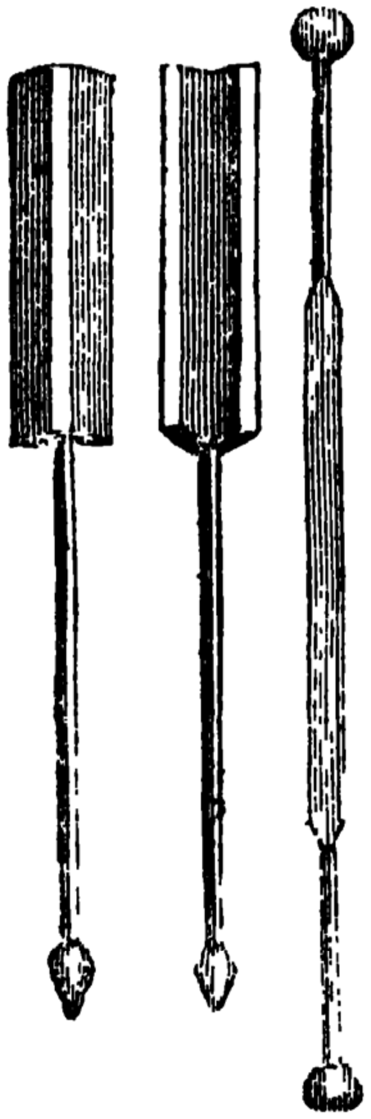


Fig. 7.

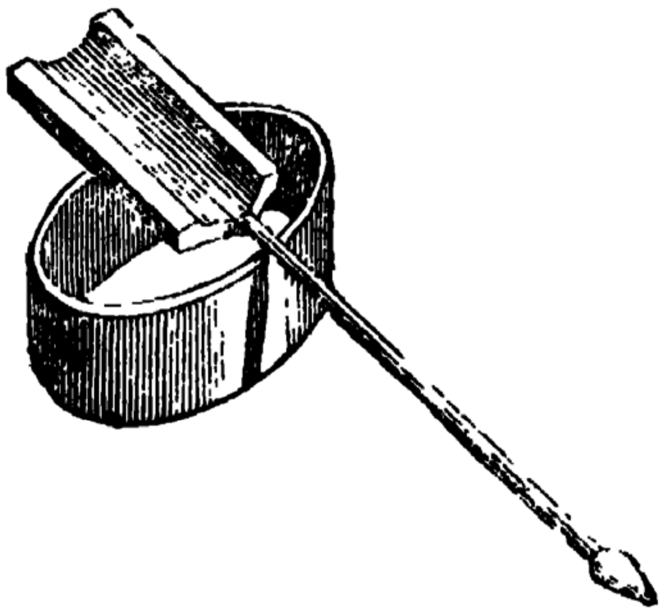


Fig. 1.

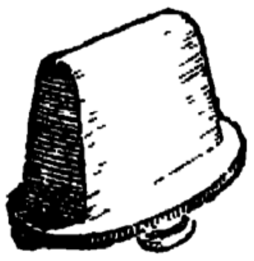


Fig. 2.

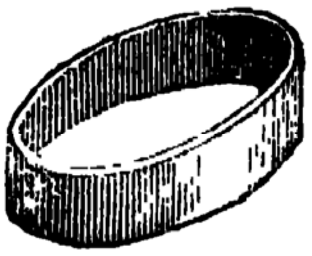
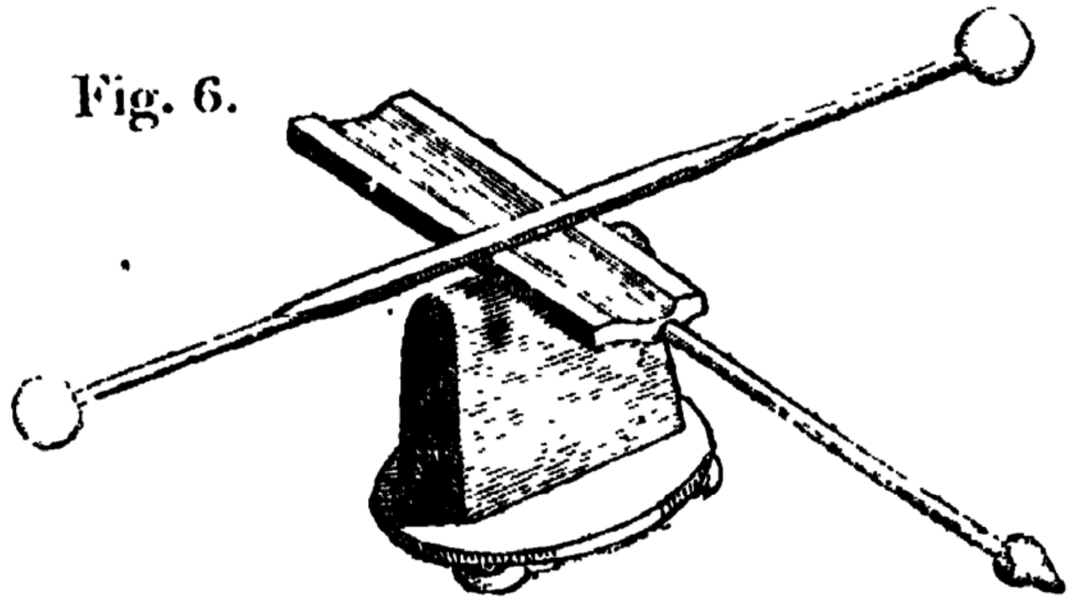


Fig. 6.

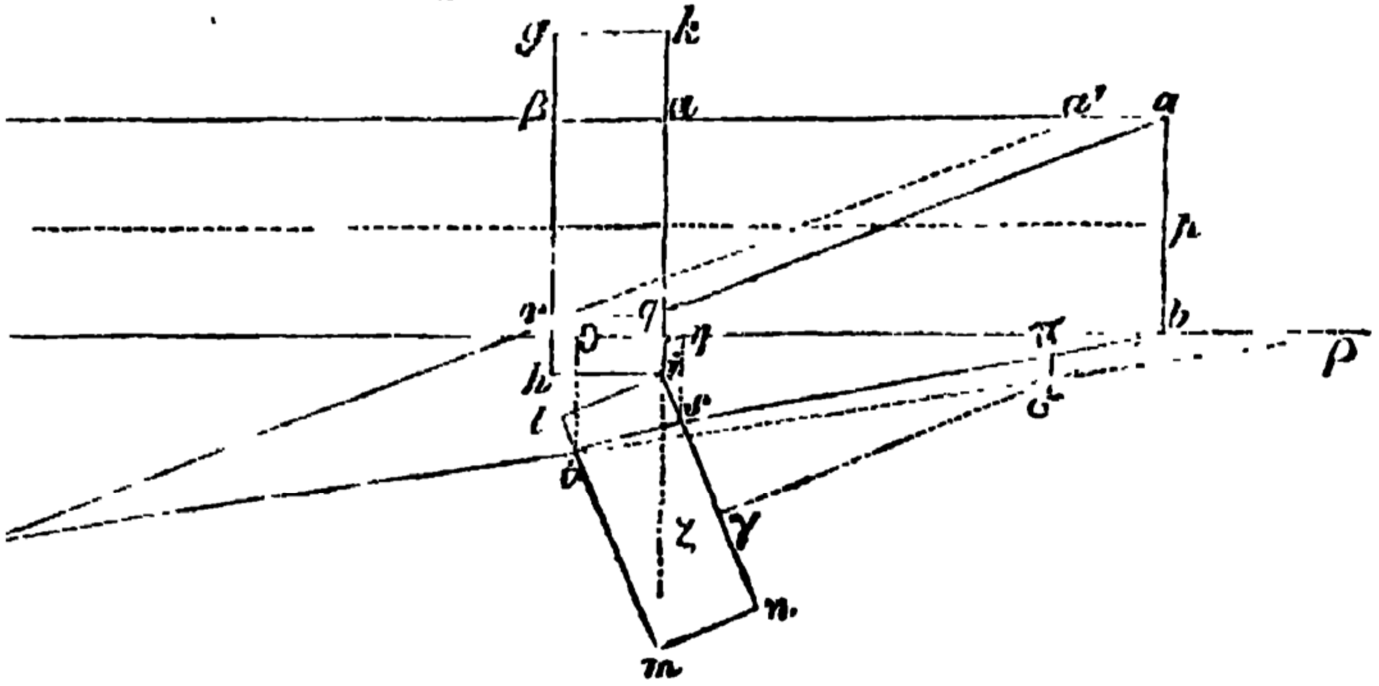


LV. *Particulars of a Series of Experiments and Calculations undertaken with a View to determine the Velocity with which Light traverses Transparent Media.* *By* R. Potter, *Jun., Esq.*[*]

BEFORE entering on the immediate subject of the essay, I must be excused a few remarks on Professor Airy's last paper, in the Phil. Mag. for June (vol. ii. p. 451). He does not inform us on what ground he came to the conclusion, that the locus of interference for any one band after prismatic refraction was exactly in the theoretical direction. His adjusting the wooden bar on which his eye-piece slided, so that a certain interference band might be seen on the wire, is not sufficient, unless the angle between the bar and the incident pencils is known, together with other data needful in a calculation which would require some nicety. This intricacy would have been avoided by using a criterion furnished by the experiment itself, such as the position of a band with respect to the diffracted fringes given by the edges of one of the mirrors, being the method which I employed. But Professor Airy does not inform us that he used any such expedient, nor that he found an aplanatic lens needful for such experiments.

To proceed to the subject of the paper; if *a b*, fig. 1, be two luminous points from which luminiferous surfaces depart

Fig. 1.



simultaneously, then the locus of the central band of interference will be always on the line *p e*, bisecting perpendicularly the line joining *a* and *b*. In this we suppose the whole path of the light to be in air. But if any other medium such as a plate of glass with parallel surfaces were placed as at *g h i k*, perpendicularly to *p e*, the place of central interference would not be altered. If, however, another pate of the same glass be placed as at *l m n i*, and inclined to the direction of the incident light, then very frequently (that is, when the apparatus used is properly adjusted and applied,) there is another set of bands seen at *f*. In this case it will be seen that the upper ray *a q r f*, will have passed through a greater distance in air than the lower ray *b s t f*. But on account of the glass *l m n i* being more inclined to the ray passing through it than the glass *g h i k* to the ray *a q r f*, it will be seen that the lower ray will have passed through a greater distance in glass. Or the sum of the distances *a q* and *r f* is greater than the sum *b s* and *t f*, and the distance in glass *s t* is greater than *q r*.

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[*] Communicated by the Author.

Now, putting $d =$ the distances in air $a q + r f$
and $d_{,} =$ ———————— $b s + t f$
and putting $D =$ the distance in glass $q r$
$D_{,} =$ ————————. $s t$

Then, when the interference at the point *f* is produced between rays which have set out *simultaneously* from *a* and *b*, putting $T =$ the whole time of passing from *a* or *b* to *f*, and
$v =$ the velocity of light in air
$w =$ ———————— in glass,

we have
$$T = \frac{d}{v} + \frac{D}{w} = \frac{d_{,}}{v} + \frac{D_{,}}{w}$$

from which we find
$$\frac{v}{w} = \frac{d - d_{,}}{D_{,} - D}.$$

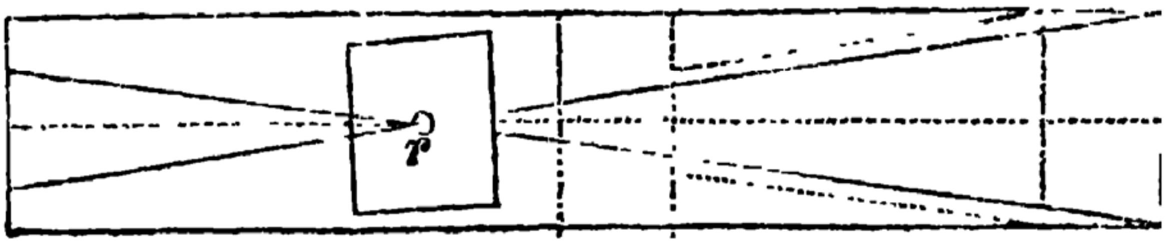
When, therefore, we can find by experiments the distances in air and glass, we can also find the relative velocities of light in them. This, however, it must be remembered, is on the supposition that the velocity is the same in glass for all incidences, and that the velocity in air is the same after having passed through the glass as before. The former of these points we shall have to refer to again presently.

In the apparatus I used for the experiments about to be described, the luminous points *a* and *b* were produced by reflections of the image of the sun given by an equi-convex lens of crown glass of $\frac{1}{10}$th inch focus, from two mirrors of speculum metal. These mirrors were set in a frame, and so that their contiguous edges could be adjusted by means of four fine-threaded screws. The glasses *g h i k*, *l m n i*, were produced by dividing a piece from one plate, which had been examined very carefully to see that the surfaces were flat and truly parallel. The mode of examining the parallelism of the surfaces which I adopted, was that of viewing the reflected images they gave of a narrow line of light in the following manner.—Standing in a room facing the window through which the light from the sky came directly, and at the distance of a few yards from this window, I held up before me the plate (a small one) with a piece of black velvet behind it; then holding a piece of fine platinum wire (a very fine steel needle would answer as well,) directly before the pupil of one of my eyes, I viewed with the same eye the images, produced by the surfaces of the glass, of the fine line of light which was shown by the platinum wire. When the surfaces are even very slightly inclined to each other, it is immediately detected by their two images being distinctly separated; but when both surfaces are accurately perpendicular to the same incident rays, the two reflected images become blended into one. Knowing it to be a very difficult task to produce two such flat and parallel surfaces on a small scale, I did not attempt to grind and polish a plate myself, but chose rather to avail myself of the kindness of a friend, to select a small plate, to suit my purpose; and I considered myself fortunate in finding *one* amongst a large number of plates in which no inclination of the surfaces could be detected in any direction. One of the best methods for examining the truth of flat surfaces, I find to be that of pressing together alternately the surfaces of the two pieces of glass, when, if there be any appretiable curvature, it will be detected by an appearance of Newton's rings; care must, however, be had that the curvature does not arise from a too great degree of pressure.

My two pieces of glass from the plate just mentioned were attached, the one to the pivot of a divided circle, and the other to the arm turning on this pivot, which carried the reading microscope. The circle was something less than 18 inches in diameter; and from the examination I made of the values of the divisions, I think the angles, in the experiments which were deduced from the divisions for even tens of degrees, could not be wrong more than five or six seconds. For the other divisions it is possible the error might be several times as much, but nothing, as I have proved, which could affect fundamentally the results. The heliostat I used was the one I have described in a former number of the Magazine, (Lond. and Edinb. Phil. Mag. and Journal, vol. ii. p. 6,) and the lens and two mirrors were attached to it, so that the line *c p*, fig. 1, might be horizontal. The plane of the circle was adjusted accurately to be parallel to this line, and perpendicular to the horizon, by three screws forming the feet of its support. The two glasses being cemented to plates of brass were also adjusted by three screws each, so as to be perpendicular to the plane of the circle. The distance *e f* in the figure was measured by means of a chased steel screw set accurately in a brass frame; this screw was set perpendicular to the horizon, and it carried up or down, when turned, a piece of hard wood, to which was attached the eye-lens with a needle-point in its focus. By means of an index and a divided card, the motion of the screw was read off to the 200dth part of a revolution.

The distance of the points *a* and *b* was determined by the method used in a similar case by M. Fresnel, namely, by placing a piece of tin-plate, so that a circular perforation in it might be on the line *e p*, as in fig. 2. A series of coloured rings round a centre, also coloured, but well defined, was in this manner seen for each luminous point *a* and *b*, and the distance *i i* of these centres was very easily and accurately determined with the screw carrying the eye-glass; from which the distance *a b* was easily deduced, when the distances *e r* and *r p* had been measured.

Fig. 2.



There are many points that require great attention in conducting the experiment, with which I must not trespass longer on the reader's patience, but will merely state that they arise from the necessary smallness of the distance *a b*, the diffracted fringes of the edges of the glasses *h i*, *l i*, the peculiar adjustments required for the mirrors, the positions of the mirrors with respect to the original luminous point, and the positions of the glasses with respect to the points *e*, *f* and *a*, *b*.

The *final* adjusting of the apparatus often required all the sun-light I could obtain, for one or two days before the experiment could be proceeded with; and this occurring every time the apparatus was altered so as to experiment under different circumstances, prevented me, even during the long succession of fine weather we had this spring, from obtaining more numerous measures. The alterations of the apparatus were to produce different values for the distance *a b*, and the proper places of the edges *h i*, *l i*, for the different angles between the glasses. For all the measures in which the glasses were inclined to each other *more* than 10 degrees, the two sets of fringes at *e* and *f* were seen, and afforded the means of taking the distances of these points with great accuracy. In the others, one set only could be seen at once, so that the lower glass had to be brought up to a parallel position with the upper one for every measure; the lower pencil passing in this case through the lower glass, for both sets of fringes. The light I used was a red obtained by extracting the colouring matter of alkanet root in oil of turpentine, and then transferring it, when filtered, by solution and quick evaporation, to Canada balsam. This coloured balsam, placed between two plane glasses and cemented round with sealing-wax, furnished a convenient way of getting light of any required purity: that which I used was such as to cause rather more than twenty bright and twenty dark bands to be visible at once; and yet in this number there was no difficulty, with a little practice, in determining with certainty which was the central one.

The following are the measures I have taken, and the results which they give.

On March 30th 1833, the angle between the glasses ($\zeta i \gamma$) being 14° 1′ 7″,

the distance *ef* was found to be ·29290 inch.

When the angle ($\zeta i \gamma$) was 15° 0′ 15″,

ef was found to be ·34320.

For both the above *i i*, fig. 2. = ·10126

e r — = 12·15 inches

r p — = 19·65 inches

whence *a b* = ·1637661.

The thickness of the glass plates was found to be ·1375 inch. Taking the refractive index of the glass (μ) at 1·495, these measures give

$$\frac{d-d_{\prime}}{D_{\prime}-D} = \frac{\cdot 002812115}{\cdot 001743651} = 1\cdot 612.$$

The second one for the angle 15° 0′ 15″ I have not calculated.

On the 6th of April, I obtained the following:

Angle $\zeta i \gamma$, fig. 1 = 18° 59′ 17′

ef — = ·540276

i i fig. 2. = ·104012

e r — = 12·15

r p — = 19·9375

whence *a b*. = ·170678

from which we find when μ is taken = 1·495

$$\frac{d-d_{\prime}}{D_{\prime}-D} = \frac{\cdot 0051619}{\cdot 0030902} = 1\cdot 6704.$$

On the 21st of April, I got the following measures:

Angle $\zeta i \gamma$, fig. 1. = 5° 0′ 6″
ef — = ·0337193
$i i$ fig. 2. = ·1125248
$e r$ — = 12·10
$r p$ — = 20·04.

This value of $i i$ being taken without removing the glasses, on account of the great labour required to re-adjust the apparatus, requires to be considered as for the distance $r p$ corrected by $a a$, fig. 2, which I calculated in the experiments taken at and after this date from the expression:

$$a a_{,} = \text{thickness of glass} \times \left(\frac{\mu-1}{\mu}\right)$$

whence $a b$ = ·1859400 and for $\mu = 1{\cdot}495$

$$\frac{d-d_{,}}{D_{,}-D} = \frac{\cdot 00037980}{\cdot 00024464} = 1{\cdot}552.$$

On the same day, I obtained the following also:

Angle $\zeta i \gamma$, fig. 1. = 9° 59′ 42″
ef — = ·1240136
$i i$ fig. 2. = ·11339
$e r$ — = 12·10
$r p$ — = 20·04

whence $a b$ = ·1873696, and when $\mu = 1{\cdot}495$

$$\frac{d-d_{,}}{D_{,}-D} = \frac{\cdot 001419883}{\cdot 00092721} = 1{\cdot}531.$$

On the 8th of May, I obtained as follows:

Angle $\zeta i \gamma$, fig. 1. = 5° 0′ 6″.
ef — = ·047953687
$i i$ fig. 2. = ·0776168
$e r$ — = 12·05
$r p$ — = 20·28

whence $a b$ = ·1303349, and taking $\mu = 1{\cdot}405$

$$\frac{d-d_{,}}{D_{,}-D} = \frac{\cdot 00037257}{\cdot 00023757} = 1{\cdot}568.$$

On May 9th, I obtained the following:

Angle $\zeta i \gamma$, fig. 1. = 5° 0′ 6″
ef — = ·04641225
$i i$ fig. 2. = ·0803153
$e r$ — = ·12·05
$r p$ — = ·20·26

whence $a b$ = ·1347321, and taking here $\mu = 1{\cdot}492$

$$\frac{d-d_{,}}{D_{,}-D} = \frac{\cdot 00037475}{\cdot 00023915} = 1{\cdot}567.$$

On May 18th, I obtained the following:

Angle $\zeta i \gamma$, fig. 1. = 17° 0′ 55″
ef — = ·4131553
ii fig. 2. = ·11176437
er — = 12·08
rp — = 20·23

Another set,

Angle $\zeta i \gamma$, fig. 1. = 16° 0′ 23″
ef — = ·44349222
ii fig. 2. = ·09132167
er — = 12·08
rp — = 20·23

On re-examining the adjustments, after taking these two sets, I found the glasses had got slightly moved. It is probable that this had occurred in taking the measures of er and rp, and therefore would not influence the others; but I have not calculated them.

On May 22nd, I obtained the following:

Angle $\zeta i \gamma$, fig. 1. = 19° 59′ 39″
ef — = ·70650
ii fig. 2. = ·0397189
er — = 12·08
rp — = 20·25

whence $ab = \cdot 1500612$; and when $\mu = 1\cdot 492$,

$$\frac{d-d_{\prime}}{D_{\prime}-D} = \frac{\cdot 00576170}{\cdot 00334260} = 1\cdot 7237.$$

On May 23rd, I obtained the following:

Angle $\zeta i \gamma$, fig. 1. = 19° 59′ 39″
ef — = ·6719927
ii fig. 2. = ·09218843
er — = 12·08
rp — = 20·29

whence $ab = \cdot 1544955$, and

$$\frac{d-d_{\prime}}{D_{\prime}-D} = \frac{\cdot 00568722}{\cdot 00335378} = 1\cdot 6958.$$

when the refractive index μ is taken $= 1\cdot 495$.

When we take $\mu = 1\cdot 492$, we find

$$\frac{d-d_{\prime}}{D_{\prime}-D} = \frac{\cdot 00570817}{\cdot 00336775} = 1\cdot 6949.$$

The reason of my having used two values of the refractive index is this. The circle being engaged with the other part of the experiments, and as I possessed no other divided instrument fit for determining the refractive power of the glass, I had recourse to Dr. Wollaston's instrument, with which I deduced 1·495 as the refractive index. When the circle was at liberty, I found this to be rather too high for the red light with which I had been experimenting, and that 1·492 was about the true refractive index for it. On going, however, through the calculation again for the experiment of the 23rd of May, the difference was so small in the results, that I have not thought it needful to re-calculate the others, and particularly as the labour is considerable.

It will be seen in fig. 1, that to find the true value of the angle *q a e*, from knowing the distances *c f*, *c a* and the thickness of the glass $\beta\,\alpha$, it is necessary to find the distance $a\,a_{,}$ from the formula $a\,a_{,} = t\left(1 - \frac{\cos\phi}{\mu\cos\phi'}\right)$, where $t = \beta\,\alpha$, $\phi =$ the angle of incidence *q a e*, and $\phi' =$ the angle of refraction. By repeating this process, we at length find the angle *q a e* to any required correctness, and then easily find *d* and D.

For the lower ray, supposing a perpendicular ($b\,\gamma$) to be let fall upon the lower glass, $\sigma\,b$ is first found by the same formula as $a\,a_{,}$, and then from it $\sigma\,\tau$ or $b\,\varrho$, as may be most convenient to use. After one or two approximations in this manner, the values of the angles *s b d* and $s\,b\,\gamma$ may be found with every accuracy that the logarithmic tables to 7 figures admit of. Knowing the angle $s\,b\,\gamma$, we soon find $t\,s = D_{,}$, then from it we obtain the value of $\theta\,\eta$, and finally, $d_{,} = s\,b + f\,t = (d\,b - \theta\,\eta) \times$ secant *s b d*.

In the calculations, I have used the logarithmic tables to 7 figures, excepting for the secants of the angles *q a e* and *s b d*, in which, from the largeness of the values of *d* and $d_{,}$ compared with their differences, I found it necessary to calculate these secants from the formula, secant $= \sqrt{1 + \tan^2}$, to 10 places of decimals.

According to the undulatory hypothesis, we should have for all incidences

$$w : v :: (D - D_{,}) : (d - d_{,}) :: 1 : \mu$$

$$\text{or,} \quad \frac{v}{w} = \frac{d - d_{,}}{D_{,} - D} = \mu.$$

We see that this is not supported by the preceding experiments, and that they deviate the more from it as the angle of incidence on the lower glass is greater. This leads us at first to suppose the velocity of light in glass to be variable for different incidences. Before concluding finally on our results, we shall find it wise on this, as on many other occasions, to doubt the infallibility of the undulationists. They tell us, it is true, that the image of the sun's disk, produced by a lens of short focal length, may be used instead of a luminous body, in *all* experiments in which it is wished to produce a series of waves diverging from a point. And the measurements hitherto taken, which have been supposed by some to demonstrate the undulatory theory, have been made with common lenses, without its being imagined there was any need of achromatic, aplanatic glasses for these delicate purposes.

Without attempting to clear the foregoing results of the effects of aberration, we may still convince ourselves by a simpler procedure, that their discordance with the hypothesis,—that the velocity of light in passing through transparent bodies is inversely as their refractive indices,—is more apparent than real.

The experiments appear to show that the velocity (w) of the light in the glass was slower as the incidence was increased, making the fraction $\frac{v}{w}$ of larger value. Then the light must also have moved more slowly in the lower glass than in the upper one, on account of its greater incidence upon it.

Let us, therefore, instead of

$$T = \frac{d}{v} + \frac{D}{w} = \frac{d_{\prime}}{v} + \frac{D_{\prime}}{w}$$

take the w in the denominator on the second side equal to $z\,w$, and we have

$$T = \frac{d}{v} + \frac{D}{w} = \frac{d_{\prime}}{v} + \frac{D_{\prime}}{z\,w}$$

and
$$z = \frac{\frac{v}{w} D_{\prime}}{(d-d_{\prime}) + \frac{v}{w} D}$$

In applying this to the experiments of the 9th of May, let us assume, as the experiments warrant us in doing, that the velocity in the glass at a perpendicular incidence is to the velocity in air in the inverse ratio of their refractive indices. We find for z the following:

$$z = {\cdot}999912$$

and from this
$$\frac{v}{z\,w} = 1{\cdot}49213$$

which is nearly, as we supposed it, at a perpendicular incidence, namely, 1·492.

For another example, the experiment of May 23rd gives

$$z = \cdot996759$$

$$\text{and} \qquad \frac{v}{z\,w} = 1\cdot49685.$$

Or, the ratio of the velocities appears still to be, when fully considered, nearly 1·492, as we have taken it to be, at an incidence nearly perpendicular.

These experiments, then, give no adequate ground for maintaining the incorrectness of the hypothesis that the velocity in refracting bodies is inversely as their refractive indices.

On a careful re-examination of the thickness of the glass, of the value of the threads of the screw by which were measured the distances *i i* and *e f*, and of the other parts of the apparatus, I could find no ground to suppose the results I had obtained were influenced materially by any other imperfection besides the aberration at the focus of the lens.

I shall consider it exceedingly important to determine whether, when an achromatic lens of short focus is used to form the luminous point, the central band of direct interference given by two mirrors is black, as it has appeared to me, and to several friends to whom I have shown it, when adequately tried with a common lens,—contrary to what the undulationists would have us believe it to be. If the central band shall still be found black, I shall submit to those who are not perfectly wedded to the undulatory theory, that it *must fall*. If, however, when adequately tried, the experiment shall exhibit the central band white, I shall consider *one of my greatest* difficulties to receiving the undulatory theory to be removed.

LVI. *Descriptions of several new British Forms amongst the Parasitic Hymenopterous Insects. By* J. O. WESTWOOD, *F.L.S. &c.*[*]

[Continued from vol. ii. p. 445.]

25. *Streblocera*, Westw.

ALYSIÆ affinis. Caput anticè bituberculatum. Antennæ ferè longitudine corporis 16-articulatæ, articulo 1mo longo (capite longiori) subtùs dente valido armato, 2do brevi apice prioris obliquè inserto, 3tio paullò majori, 4to etiam obliquè inserto, hoc et reliquis filiformibus. Collare angustum. Abdomen subrhomboideum pedunculo brevi. Alæ anticæ stigmate magno, areolâ 1 marginali brevi, areolâ 1mâ submarginali cum discoidali effusâ.—*Strebl. fulviceps*, Westw. Piceo-nigra, nitida, capite fulvo, oculis ocellisque nigris, antennis fuscis, articulis 3 basalibus fulvis, pedibus fuscis, femoribus

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[*] Communicated by the Author.

fulvescentibus; alarum stigmate nervisque pallidè fuscis. Long. Corp. lin. 1. Coombe, Aug. 1833.

26. *Basalys*, Westw.

Aneurhynchum (antennis) cum *Spilomicro* (alis) arctè conjungens. Os haud rostratum. Antennæ ♂ 14-articulatæ, articulis 2 et 3 brevibus, 4to magno, externè producto, reliquis subæqualibus tenuibus. Alæ magnæ nervis duobus subcostalibus tertiam alæ partem accedentibus, nervo transverso illorum apicem connectente cum nervo ex illorum medio exeunti areolam triangularem formantibus stigmate magno internè retro-producto, nervisque quibusdam longitudinalibus ferè obliteratis.—*Bas. fumipennis*, Westw. Niger, nitidus, pilis longis ornatis, alis fuscescentibus, stigmate nervoque transverso nigris, nervis reliquis fuscis, apice femorum tibiarumque piceo, tibiis piceo-nigris. Long. lin. 1¾. Prope Londinum. R. Lewis.

27. *Elasmus*, Westw.

Ex *Eulopho typicali* differt antennis ♂, articulis 3, 4 et 5 brevissimis ferè coalitis (singulo ramum longum emittenti), 6to longo, reliquis clavam formantibus; ♀ ut videtur 10-articulatæ, 3 et 4 annuliformibus, 5, 6, 7 oblongis, 8, 9, 10 clavam formantibus, metathorax (♂ ♀) utrinque ad latera in laminam magnam concavam posticè productus.—Alæ angustissimæ. Pedes longissimi femoribus magnis.—*Eulophus flabellatus*. Fonscol. Ann. Sc. Nat., Jul. 1832. (Descr. antennarum vitiosa). Coombe, Aug. 1833.

28. *Stenomesius*, Westw.

Euplectro affinis. Caput lutiusculum. Antennæ longiores, ♂ graciles, ♀ subincrassatæ, 9-articulatæ, articulo 2do 3tio dimidio breviori, clavâ 3-articulatâ. Thorax anticè collari attenuatus; abdomen pedunculo brevi, ♂ parvum spatuliforme, ♀ majus, ovatum. Alæ magnæ. Pedes sat elongati tibiis intermediis, ♂ attenuatis, apice subclavatis.—1. *Sten. pulchellus*, Westw. ♂. Capite nigro, antennis nigris, basi subtùs pallidis, collari et mesothorace fulvo-testaceis, parapteris nigris metathorace nigro, abdomine nigro, basi maculâ magnâ fulvâ, pedibus fulvis, anticorum femoribus basi tibiisque apice fuscis, intermediorum tibiis apice tarsisque totis nigris; alis immaculatis. Variat collari scutelloque plùs minùsve nigro notatis. Long. lin. ⅔. Exp. Alar. 1⅓. Prope Londinum et Cantabrigiam.—2. *Sten. maculatus*, Westw. ♀, Capite fulvo maculâ occipitali lineâque pone oculos nigris, antennis nigris articulo basali fulvo, thorace ut in præcedenti colorato, abdomine nigro basi fasciâ tenui lineâque brevi fulvis, pedibus fulvis, tarsorum apice fusco, alis maculâ centrali fuscescenti. An ♀ præcedentis? Long. lin. 1. Exp. Alar. 1⅞. Prope Londinum. R. Lewis.

29. *Cheiloneurus*, Westw.

Encyrtum (pedibus) cum *Eupelmo* (alis) conjungens. Caput sat magnum. Mandibulæ 3-dentatæ. Antennæ prope os insertæ, ferè thoracis longitudine, versus apicem subclavatæ, 11-articulatæ, articulo 2do 3tio majori. Thorax oblongo-quadratus, anticè collari attenuato, scutello posticè fasciculato; abdomen ovatum depressum posticè acuminatum. Alæ anticæ nervo stigmaticali brevissimo à loco conjunctionis nervi subcostalis cum costâ paullò remoto. Pedes ut in Encyrto.—*Cheil. elegans*, Dalm. (*Encyrtus* et *Eupelmus*). Capite obscurè æneo, oculis antennisque nigris; his basi et apice pallidioribus, thorace cinerascenti-æneo, scutello flavo, abdomine nigro, cupreo nitenti, alis fuscis, in medio obscurioribus, basi atque prope et sub stigmata albis, pedibus pallidè testaceis, femoribus tibiisque posticis obscuris, tibiarum 4 posticarum basi albo. Long. Corp. lin. ⅘. Expans. Alar. lin. 1⅓. Richmond Park, Aug. 1833.

30. *Ectroma*, Westw.

Encyrtum (pedibus) cum *Theocolace* (habitu) conjungens. Caput crassum mandibulis 3-dentatis. Antennæ inter partem inferiorem oculorum insertæ, ferè corporis longitudine, sensim incrassatæ, 9-articulatæ, articulo 3tio 2do minori, 9no 8vo haud majori. Thorax oblongo-quadratus collari acuminato. Alæ rudimentales.—*Ectr. fulvescens*, Westw. Capite thoraceque pallidè fulvis, æneo submicantibus, abdomine obscurè testaceo metallico, oculis et antennis nigris, his basi subtùs, apiceque articuli terminali pallidis pedibus fulvis, tarsorum apice nigro. Long. lin. ⅔. Coombe, Aug. 1833.

31. *Pteroptrix*, Westw.

Agonioneuro affinis. Corpus brevissimum, latum, depressum. Caput transversum oculis magnis pilosis. Antennæ 8-articulatæ, articulis 2 et 3 æqualibus, 4to paullò minori, 5, 6, 7 multò majoribus, 8vo minutissimo. Thorax ferè quadratus. Scutellum magnum. Alæ magnæ, apice longè pilosæ, nervo stigmaticali brevi angulum acutum formante. Abdomen sessile, brevissimum, apice mucronatum. Tarsi 4-articulati.—*Pter. dimidiatus*, Westw. Piceo-niger, scutello albido, antennis flavis, articulo 7mo obscuriori, alarum dimidio basali fusco, pedibus pallidis, femoribus tibiisque basi fuscis. Long. Corp. lin. ¼. In quercu, Richmond Park, Aug. 1833.

32. *Coccophagus*, Westw.

Agonioneuro affinis, differt antennis 8-articulatis, articulo 2do 3tio minori, hoc et duobus sequentibus ferè æqualibus, ultimis 3iis clavam, vix articulo præcedenti crassiorem, formantibus. Tarsi 5-articulati; nervus stigmaticalis brevis apice conoideus. Habitat in Coccis.—Sp. 1. *Entedon scutellaris*, Dalm. Sw. Tr. 1825. 365*.—Sp. 2. *Cocc. pulchellus*, Westw. Pallidè flavus, oculis, thorace anticè abdomineque nigris, antennarum flagello thoracisque lateribus obscurioribus, pedibus flavis, tarsorum apice fusco. Long. lin. ½. Exclusus e *Cocc. aceris*, Jun. 1833.—Sp. 3. *Cocc. obscurus*, Westw. Niger, abdomine nitido, antennis fuscescentibus, pedibus sordidè albidis, tarsorum apice fusco, femoribus intermediis basi, posticis totis, tibiisque posticis basi fuscis. Alarum nervo fusco. Long. lin. ⅔. Habitat cum præcedenti. Sp. 4. *Entedon insidiator*, Dalm. l. c. 371.

LVII. *Characters of some undescribed Genera and Species of* Araneidæ. *By* John Blackwall, *Esq. F.L.S. &c.*†

Tribe, INEQUITELÆ, Latreille.